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# WORLD POLITICS

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# ABSTRACTS

## Supportive Participation with Economic Growth: the case of Japan

By I. KABASHIMA

Whether political participation is positively related to economic equality is still a paradox. This paper explores the relations among political participation, economic equality, and economic development in Japan after World War II. There was little income bias in political participation in Japan during its period of rapid growth, partly because farmers who benefited inadequately from economic development participated more in politics. This rural bias in participation allowed significant redistribution of income from the urban to rural sector through the budgeting system, thus preventing the natural tendency of widening income inequality at the early stage of development. However, high rural participation did not undermine the rate of growth because it was supportive participation. Farmers overwhelmingly supported the incumbent Liberal Democratic Party, support that enhanced government's continuity and in turn nurtured growth.

## Intelligence and Warning: implications and lessons of the Falkland Islands War

By G. W. HOPPLE

Was the Falkland conflict either preventable or predictable? Based on the available evidence, it is apparent that the British could not have been expected to foresee the outbreak of the war. Both Argentina and Britain relied on simplistic and misleading preconceptions or strategic assumptions, leading to a war that might have been avoidable. Issues of conflict and crisis warning are illuminated in this essay, and the Falklands War is related to past instances of "warning failure" and surprise attack. The crucial importance of the strategic logic of decision makers is emphasized. In addition, the lessons of the war for the 1980s highlight the changing nature of the international systemic context and the heightened prospects for crisis and conflict both between allies and within the third world.

## Secularization and States of Modernity

By T. G. CARROLL

Extensive secularization is frequently held to be a necessary condition for political modernity. The author argues that the relationship between religion and the modern state is considerably more complex than this general proposition suggests. It is necessary to specify particular ideological models of the modern state, since these differ significantly from one another; and it is necessary to specify particular religions in their contemporary manifestations, since these also differ in important ways. A detailed analysis of this type suggests that there is no general incompatibility between the main religions of the third world and widely shared, nonideological features of political modernity. Specific religions are shown to be incompatible with some specific forms of the modern state, while presenting no significant obstacle to other models of political modernity.

## International Relations Theory, Foreign Policy Substitutability, and "Nice" Laws

By B. A. MOST AND H. STARR

Two logical problems appear to have impeded the development of an integrative understanding of international and foreign policy phenomena. The first has to do with the potential for foreign policy substitutability: through time and across space, similar factors could plausibly be expected to trigger different foreign policy acts. The second concerns the potential existence of "sometimes true," domain-specific laws. It is the logical opposite of the substitution problem, suggesting that different processes could plausibly be expected to

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lead to similar results. Neither problem appears to be well understood in the current literature; if anything, both are ignored. Nevertheless, they are potentially important. Together, they suggest that scholars who are interested in developing a cumulative base of integrative knowledge about foreign policy and international relations phenomena need to rethink both their focus on middle-range theory and their application of the standard approaches. We recommend reconsideration of some of the "grand" theoretical approaches found in the "traditional" literature. A new synthesis of tradition and science and of grand, middle, and narrow approaches appears to be needed. Finally, in contrast to the arguments of proponents of a systems-level approach, we argue that the most fruitful avenues for theorizing and research are at the microlevel in which the focus is on decision making, expected utility calculations, and foreign policy interaction processes.

## War and Expected-Utility Theory

By R. H. WAGNER

In his book *The War Trap* Bruce Bueno de Mesquita claims to offer a deductive theory of international conflict based on the assumption that foreign policy makers attempt to maximize their expected utility. The theory is subjected to a systematic empirical test that seems to provide impressive confirmation. The first part of this article examines Bueno de Mesquita's theory and argues that it cannot be derived from his assumptions. The second part examines the operational version of the theory, which was the one actually tested, and argues that it can be more plausibly interpreted as a version of a different theory—one that Bueno de Mesquita claims to have discredited. The essay concludes by discussing the significance of this book with regard to the question of whether theories of individual rational choice can explain foreign policy decisions.

## Advances in the Comparative Study of Electoral Systems

By A. LIJPHART

The twelve books under review, written by scholars representing many different disciplines and nationalities, are proof that the comparative analysis of electoral systems has made significant progress in the late 1970s and early 1980s. It is still not a well-developed field, but it has clearly become a less underdeveloped one. Renewed interest in research on electoral systems has been stimulated by major changes in election rules—usually in the direction of proportional representation—that have been adopted in several countries, and by a vigorous debate on electoral reform in countries that now rely mainly on the plurality method. The United States is the principal deviant case. Two election systems frequently serve as models for electoral reform: the Irish single transferrable vote and the West German additional-member system.

## Failure, Defeat, Debacle: U.S. Policy in the Middle East

By L. BINDER

Revolution, war, and political stalemate in the Middle East have led many analysts to declare U.S. policy in the Middle East a failure. To a considerable extent this failure is attributed to an unwillingness to use the area experts who have the requisite knowledge. Often, however, knowledge, intelligence, and analysis are conflated. Frequently, expertise and advocacy are confused. In practice it is difficult to separate scientific knowledge from partisan ideological commitment. Hence the close association between government and the social scientific/area studies community may well defeat the purpose of providing objective and institutionally neutral bases for policy making. Despite some recent trends toward linking the enhanced funding of area studies with more direct service of the needs of government agencies, it may actually be more desirable to explore better ways of detaching area studies from the institutional establishment and the policy orientations of the current National Defense Education Act system.

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# SUPPORTIVE PARTICIPATION WITH ECONOMIC GROWTH: The Case of Japan

By IKUO KABASHIMA*

## Introduction

THE simultaneous advancement of economic development, socio-economic equality, and democracy in terms of broad-based political participation is a difficult goal for political elites because these three variables are not readily compatible. According to Huntington and Nelson, political elites in developing countries must choose between two conflicting paths in the later phase of development: the technocratic model and the populist model.[1] On the first developmental path, political elites repress political participation in order to promote economic development; as a result, economic inequality may increase. On the second path, the elites allow more political participation, promoting economic equality through governmental programs that aim at economic redistribution; the rate of growth, however, may be slowed down. These classifications of developmental paths raise the question of whether broad-based political participation and economic equality are necessarily incompatible with economic growth.

After World War II, Japan followed a developmental path that was at variance with both of those suggested by Huntington and Nelson. In the wake of rapid economic growth, it experienced a narrowing of income inequality.[2] Moreover, Japanese participatory opportunities have opened widely since the war. The Japanese experience suggests that the relationships among rapid growth, economic equality, and the expansion of political participation are not necessarily zero-sum games. Still, several

* Research for this study was supported by a Harvard Yenching Institute Junior Fellowship for 1977–1979 and the Japanese Ministry of Education's Grant-in-Aid for Scientific Research for 1982–1983. I am grateful to Sidney Verba, Edwin O. Reischauer, and Samuel P. Huntington (Harvard University) and Ichiro Miyake (Doshisa University) for their comments on my Ph.D. dissertation on which this paper draws, and also to Terry E. MacDougall (Harvard University) for his comments and encouragement.

[1] Samuel P. Huntington and Joan M. Nelson, *No Easy Choice: Political Participation in Developing Countries* (Cambridge: Harvard University Press, 1976), 17-29.

[2] Martin Schnitzer, *Income Distribution: A Comparative Study of the United States, Sweden, West Germany, the United Kingdom, and Japan* (New York: Praeger, 1974), 232.

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important questions remain unanswered: What is the role of political participation in determining this unique pattern of development? More specifically, what was the structure of political participation in Japan during the period of rapid growth? Did participation matter in reversing a natural tendency toward increased income inequality in the rapid-growth stage of the economy? Why did the expansion of political participation not deter political elites from promoting economic development?

Although we may assume that the expansion of political participation promotes income equality, previous studies of the effect of participation on economic equality have suggested that the former has little effect on the latter.[3] Jackman did find a significant bivariate relationship between democracy and socioeconomic equality; but he discovered this relationship to be spurious because economic development was positively related to *both* socioeconomic equality and democratic performance.[4] Jackman's index of democratic performance places the emphasis on the expansion of democratic rights such as the right to vote, competitiveness of the party system, electoral regularity, and freedom of the press. Why is democratic performance, when measured in this manner, not related to socioeconomic equality?

Verba and Nie suggest an explanation. Their fundamental assumption is that the socioeconomic hierarchy of a society corresponds with its political hierarchy.[5] Under this assumption, the expansion of political rights will not automatically result in economic equality because the have-nots simply do not exercise their participatory opportunities as much as the haves. "Equal rights to participate are just that: rights, not obligations."[6] In other words, Verba and Nie suggest a positive relationship between equality and income neutrality in participation, but not between equality and the expansion of democratic rights *per se*. Income neutrality in political participation refers to the extent to which the economically disadvantaged groups in a society reveal their preferences in the political system through various political acts, while the other conditions of democracy—such as the right to participate in politics, electoral regularity, freedom of the press, and the competitiveness of the party system—are held constant. The basic hypothesis of the

[3] Irma Adelman and Cynthia Taft Morris, *Economic Growth and Social Equality in Developing Countries* (Stanford, Calif.: Stanford University Press, 1973); Robert W. Jackman, *Politics and Social Equality: A Comparative Analysis* (New York: Wiley, 1975).

[4] Jackman (fn. 3), 68-89.

[5] Sidney Verba and Norman H. Nie, *Participation in America: Social Equality and Political Democracy* (New York: Harper & Row, 1972).

[6] *Ibid.*, 335.

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present study is that there was little income bias in political participation during Japan's period of rapid growth, and that this income neutrality in participation is related to an equitable income distribution in the course of economic development.

The specific objectives of this essay are:

1. To analyze the structure of Japanese political participation during the country's rapid-growth stage. To what extent is the participatory structure income-neutral, and what social and demographic variables reinforce income neutrality in political participation?
2. To relate the participatory structure to the postwar trend in income distribution. What is the role of participation in determining an equitable growth pattern, and how are the expansion of political participation and rapid economic growth compatible?
3. To discuss the findings in a comparative perspective.

## The Structure of Political Participation

Our first task is to analyze the structure of Japanese political participation. The present analysis is based on data collected in 1966, at the height of Japan's period of rapid economic growth, by the Cross-National Program in Political and Social Change headed by Sidney Verba.[7] Table 1 presents a comparison of the extent of income elasticity in different modes of political participation for seven countries.

The indices of different modes of political participation are the same as those constructed by Verba, Nie, and Kim.[8] The income figure is based on the respondent's family income before taxes. Relatively speaking, the structure of participation in Japan is quite income-neutral. Income elasticity in voting, campaigning, and communal activities is 0.06, 0.13, and five of the other countries; it is lower still for Austria and India. Not only is the coefficient small; it is also not statistically significant at an 0.05 level. Japan's income elasticity in campaign activity is the lowest of the seven countries; in communal activity, it is comparable to India, lower than the United States, the Netherlands, and Nigeria, and higher than Yugoslavia and Austria.

The income elasticity shown in Table 1 neglects the influence of politically relevant subpopulations. In the situation shown in Figure 1-

[7] For the nature of the program and sampling design, see Sidney Verba, Norman H. Nie, and Jae-On Kim, *Participation and Political Equality: A Seven-Nation Comparison* (New York: Cambridge University Press, 1978), xi-xxi, 349-75.

[8] *Ibid.*, 340-48.

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TABLE 1

INCOME ELASTICITY IN THREE MODES OF POLITICAL PARTICIPATION IN YUGOSLAVIA, U.S.A., JAPAN, INDIA, AUSTRIA, NIGERIA, AND THE NETHERLANDS

| | *Voting* | *Campaign Activity* | *Communal Activity* |
|---|---|---|---|
| Yugoslavia | 0.19 | 0.57 | 0.17 |
| | (24) | (81) | (8) |
| U.S.A. | 0.50 | 0.47 | 0.48 |
| | (121) | (94) | (90) |
| Japan | 0.06 | 0.13 | 0.36 |
| | (1) | (4) | (32) |
| India | 0.03 | 0.52 | 0.34 |
| | (1) | (152) | (71) |
| Austria | −0.06 | 0.28 | 0.29 |
| | (1) | (11) | (22) |
| Nigeria | 0.23 | 0.42 | 0.42 |
| | (9) | (28) | (28) |
| Netherlands | 0.19 | 0.25 | 0.54 |
| | (4) | (7) | (31) |

Note: The term "income elasticity" indicates the percent change in the rate of political participation for every percent change in income. It was estimated by a simple regression model in the log-log form. The figure in parentheses is an F-ratio which provides a testing of the significance of the coefficients. If the F-ratio is more than 4, it is statistically significant at an 0.05 level of significance.
Source: Survey data collected in 1966 by the Cross-National Program in Political and Social Change.

A, subpopulation X clusters around the upper left-hand side of the income-participation dimension.

The positive slope line, I, shows the effect of income on participation independent of subpopulation X (hereafter referred to as partial income elasticity). The main feature of subpopulation X is that the mean level of income is low, whereas the mean rate of political participation is high. Because of the presence of subpopulation X, the left-hand side of line I is pulled up so that the observed regression line is much flatter (i.e., it indicates a smaller income bias).

The location of subpopulations is not limited to the upper left-hand corner. Figure 1-B shows alternative locations of subpopulations. Subpopulation A pulls up the right-hand side of line I, while subpopulation B pulls it down. Subpopulation A reinforces income bias in participation because both the mean of income and the rate of participationn are

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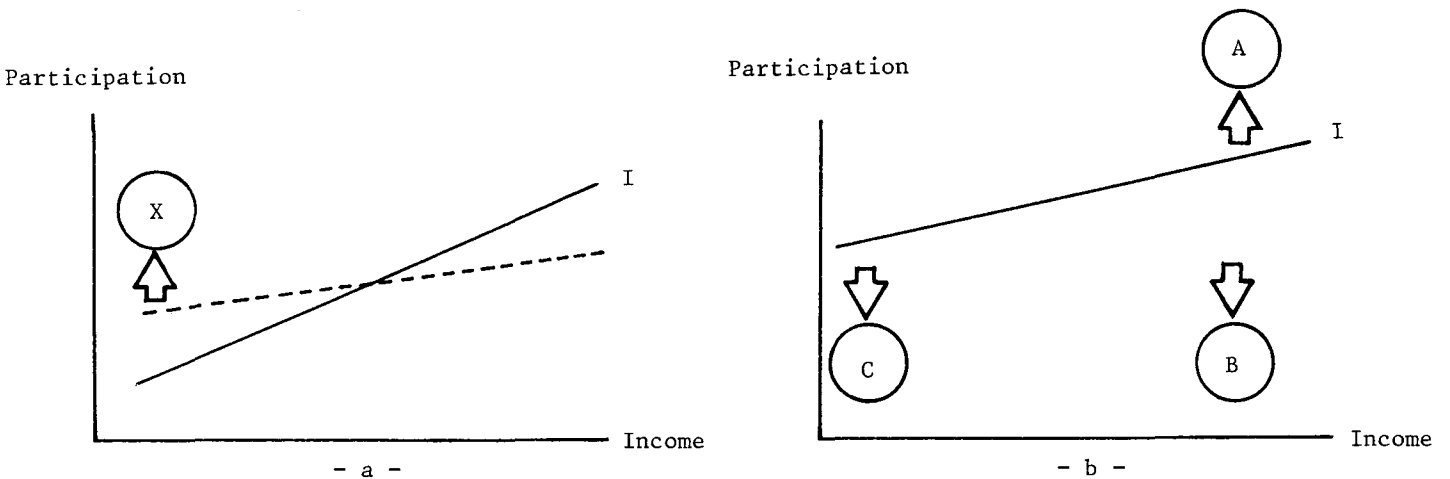
FIGURE 1

THE RELATIONSHIP BETWEEN PARTICIPATION AND INCOME AS AFFECTED BY DIFFERENT SUBPOPULATIONS

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high; the combination steepens line I. Subpopulation B neutralizes income bias in participation, whereas subpopulation C reinforces the bias. Thus, the figures in Table 1 represent the combination of effects of income and other sociological variables.

A diagrammatic presentation of sociological sources of income bias in participation can be described by statistical equations.[9] The log-linear relationship between t's political participation ($P_t$) and income ($I_t$) can be shown as

$$\ln P_t = \alpha_o + \alpha_1 \ln I_t + u_t \qquad (1)$$

where $\alpha_o$, $\alpha_1$, and $u_t$ indicate a constant, income elasticity of participation, and a residual, respectively. We assume that $u_t$ is not correlated with $\lambda n I_t$. However, $P_t$ is not only dependent on $I_t$, but is also related to the set of other sociodemographic variables denoted by $S_t$.

$$\ln P_t = \beta_o + \beta_1 \ln I_t + \sum_{j=2}^{k} \beta_j \ln S_{jt} + \epsilon_t \qquad (2)$$

The income bias in participation ($\alpha_1$) defined in equation (1) is related to the behavioral parameters defined in equation (2) by

$$\alpha_1 = \beta_1 + \sum_{j=2}^{k} \beta_j \gamma_j \qquad (3)$$

where $\gamma_j$ is the elasticity of a sociodemographic variable with respect to income. Equation (3) thus states that the extent of income bias in participation is the combination of:

1. the partial income elasticity of participation, $\beta_1$;
2. the elasticity of participation with respect to sociodemographic variables, $\beta_j$; and
3. the elasticity of sociodemographic variables with respect to income, $\gamma_j$.

By estimating $\beta_1$, $\beta_j$, and $\gamma_j$ individually, we are able to ascertain sociological sources of income bias in participation.[9]

[9] I learned a great deal from Martin S. Feldstein, "Wealth Neutrality and Local Choice in Public Education," *American Economic Review* 65 (March 1975), 75-89, for constructing the following model. For purposes of estimation, it is convenient to replace equation (3) by the more explicit system of equations $\ln S_{jt} = \alpha_j + \gamma_j \ln I_t + u_{jt}$, $j = 2, \ldots k$, where error terms $u_{jt}$ are assumed to be mutually independent and each independent of $\epsilon_t$ in equation (2). By differentiating each of these equations with respect to income, one may readily verify that each $\gamma_j$ is precisely the elasticity of $S_{jt}$ with respect to $I_t$. Hence these equations together with (2) are equivalent to (1), (2), (3), and are seen to define a *recursive* system of linear equations within which the parameters $\beta_1, \beta_2, \ldots \beta_k$ and $\gamma_2, \ldots \gamma_k$ can be consistently estimated by ordinary least squares. See Phoebus J. Dhrymes, *Econometrics: Statistical Foundation and Application* (New York: Springer Verlay, 1974), 307.

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This approach is very similar to that of Verba and Nie.[10] In Part II of *Participation in America*, their analysis begins with the standard socioeconomic status model of political participation—i.e., the higher the socioeconomic status of citizens, the more they participate in politics. The authors go on to ask what variables would neutralize this socioeconomic status bias in the United States. Their approach to assessing the magnitude of the neutralizing force of the variables involves the following questions:

1. What is the effect of a particular variable on the rate of political participation, independent of socioeconomic status?
2. Is this impact greater for citizens of lower socioeconomic status or for citizens of higher socioeconomic status?
3. How accessible is this variable to citizens of lower socioeconomic status?
4. What is the total effect of this variable on socioeconomic status bias in political participation?

Obviously, Verba and Nie's standard socioeconomic status model of political participation is comparable to $\beta_t$ in our model; their first, third, and last steps are comparable to $\beta_j$, $\gamma_j$, and $\beta_j\gamma_j$ of the income bias model, respectively.

So far, we have not defined sociodemographic variables ($S_t$) relative to the income bias model. We hypothesize that income, education, organizational involvement, urbanism, and *Sokagakkai* membership are related to the income bias model.[11] The educational figure is based solely on the last grade in school completed by the respondent: since we assume that college graduates behave differently from the rest of the population because they have different social training, in the model this variable is independent of the educational dimension. The organizational involvement index is based on a scale of 1 for respondents who are not members of any organization; 2 for passive members; and 3 for active members, regardless of the number and kind of organizations to which respondents belong. Urbanism is measured by the size of the community in which the respondent lives. Although Japan is essentially a homogeneous society with little ethnic and religious conflict, *Sokagakkai*, the lay association of the *Nichiren Shoshu* sect of Buddhism, grew rapidly after World War II in the urban areas, drawing its members mainly from the lower socioeconomic section of the population. We expect *Sokagakkai* mem-

[10] Verba and Nie (fn. 5), Part II.

[11] We included age and gender variables in equation (2), but excluded them from equation (3) because they were assumed to be unrelated to family income. Empirical estimation of the correlation between age and family income was $-0.04$, and that between gender and family income was $-0.05$, respectively. Neither was statistically significant at an 0.01 level.

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bership to be an important variable in relation to the income bias model, neutralizing in its effect. Table 2 presents the estimates of coefficients of the income bias model.

The total income bias in political participation at the bottom of Table 2 is obtained by adding up the figures in the $\beta_i\gamma_i$ columns. When F-values of both $\gamma_i$ and $\beta_i$ are greater than 4, the figures are in a box; thus, figures in a box imply that they are statistically significant at an 0.05 level.

The data show that the largest component of income bias in participation is a partial income elasticity ($\beta_i$). The partial income elasticities in voting, campaign activity, and communal activity are 0.11, 0.16, and 0.32 respectively.

Education tends to reinforce income bias in political participation, except for voting. There is a significant positive relationship between income and education, and the independent effects of education on campaign and communal activities are positive; the combination reinforces income bias in these modes of participation. The educational effects on campaign and communal activities are not statistically significant, however, and are even negative in the case of voting. A similar tendency operates with respect to college graduation. Graduation from college might be expected to reinforce income bias in participation, but the results run counter to our expectations. The relationship between income and this variable is significantly positive, but the independent effects of college graduation on voting and communal activity are not statistically significant; on campaign activity, they tend to be negative.

These findings are not consistent with the generalization that educated people participate more in politics than others.[12] Highly educated persons generally participate more in politics because they are more interested in politics, have more information about politics, and think themselves capable of influencing the government. A separate analysis shows that the elasticities of political interest, political information, and political efficacy with respect to education were 0.59, 0.55, and 0.30, respectively.[13] These findings suggest that highly educated persons are more likely to express high civic attitudes, but these attitudes do not necessarily mobilize the Japanese to participate. According to Milbrath and Goel, a higher level of education does not lead to greater patriotism and system affection in some societies; this negative feeling toward the political system is part

[12] See Lester W. Milbrath and M. L. Goel, *Political Participation*, 2d ed. (Chicago: Rand McNally, 1977), 98-102, for a summary of literature on this subject.

[13] Ikuo Kabashima, "Political Participation and Income Distribution in Growing Economies," Ph.D. diss. (Harvard University, 1979).

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TABLE 2

THE SOCIOLOGICAL SOURCES OF INCOME BIAS IN THREE MODES OF POLITICAL ACTIVITY IN JAPAN

| | | *Voting* | | *Campaign* | | *Communal* | |
|---|---|---|---|---|---|---|---|
| *Variables* | $\gamma_i$ | $\beta_i$ | $\beta_i\gamma_i$ | $\beta_i$ | $\beta_i\gamma_i$ | $\beta_i$ | $\beta_i\gamma_i$ |
| Income | 1.00 | 0.11 (3.8) | 0.11 | 0.16 (5.6) | 0.16 | 0.32 (23.0) | 0.32 |
| Education | 0.22 (248) | −0.01 (0.0) | — | 0.11 (0.8) | 0.02 | 0.12 (1.0) | 0.03 |
| College graduate* | 0.10 (129) | 0.04 (0.1) | — | −0.03 (0.0) | — | 0.28 (2.2) | 0.03 |
| Organization | 0.04 (8) | 0.61 (44.8) | 0.02 | 1.22 (123.6) | 0.05 | 1.38 (165.2) | 0.06 |
| Urbanism | 0.52 (59) | −0.13 (51.7) | −0.07 | −0.18 (68.9) | −0.09 | −0.15 (51.4) | −0.08 |
| Sokagakkai* | −0.02 (11) | 0.29 (2.3) | −0.01 | 0.41 (3.2) | −0.01 | −0.39 (3.0) | 0.01 |
| Total income bias | | | 0.05 (0.06) | | 0.13 (0.12) | | 0.37 (0.36) |

Note: Figure in parentheses is an F-ratio. Figure in parentheses under total income bias is the income elasticity estimated by equation (1).

* The coefficients of these variables measure the percentage of change in dependent variables due to one unit of change in independent variables.

Source: Survey data collected in 1966 by the Cross-National Program in Political and Social Change.

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of the reason why highly educated people in Japan do not participate more in politics.[14] It is beyond the scope of the present study to seek the reason for the lack of a significant positive relationship between education and participation in Japan.

Among various sociological variables, the most important one that reinforces income bias in political participation is organizational involvement (OI). Organizational involvement significantly reinforces income bias in all modes of participation, accounting for approximately 18, 31, and 19 percent of the partial income elasticity in voting, campaign, and communal activities, respectively. The significant biasing effects of OI derive more from the high independent effect of this variable on participation than from the relationship between income and organizational involvement. This finding leads us to the proposition, similar to one made by Verba and Nie, that OI is an important "potential" source of narrowing the participation gap between the haves and the have-nots.[15] Since the elasticity of OI is great, a small change in the OI/income relationship could significantly alter the effect of OI on the extent of income bias in participation. If the OI/income relationship were to be negative (e.g., recruiting the have-nots to organizations), OI could neutralize income bias in participation. The next variable, *Sokagakkai* membership, does in fact play this role.

As already mentioned, the *Sokagakkai* recruits its members mainly from the lower socioeconomic sectors of the population. Hence, our data reveal a negative relationship between membership in this organization and income level. *Sokagakkai* is successful (though this is not statistically significant) in mobilizing its members in voting and campaign activities. But it does not mobilize people to participate in communal activities. Consequently, *Sokagakkai* tends to play a neutralizing role on income bias in voting and campaign activity, but a reinforcing role on income bias in communal activity.

A notable variable in the income-bias model is the degree of urbanism. It neutralizes income bias in all modes of political participation in Japan, registering 63, 56, and 25 percent of the partial income elasticity in voting, campaign, and communal activities, respectively: while there is a positive relationship between income and city-dwelling, there is a negative relationship between city-dwelling and participation. This result is counter to some cross-cultural findings with respect to the relationship between urbanism and participation. A possible explanation for the significant negative relationship between urbanism and political ac-

[14] Milbrath and Goel (fn. 12), 101.
[15] Verba and Nie (fn. 5), 208.

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tivity in Japan is that rural residents are mobilized to participate in politics because they live in dense social and geographical networks and abide by authoritarian traditions requiring deference to leaders.[16] To explore why rural residents in Japan participate more in politics than urban ones, and to provide an alternative explanation, is, again, beyond the scope of the present study. Rather, in the next section we will examine the consequences of this rural bias in participation in relation to the pattern of income distribution accompanying economic development.

A summary of our analysis up to this point and an explanation of its implication for the forthcoming analysis are in order. Thus far, we have shown that the extent of income bias in participation in Japan is considerably lower than in the other countries included in our data. As we hypothesized, income bias in Japan is based on a combination of (1) an independent income effect and (2) various income-biasing and neutralizing effects of sociological variables. Among different sociological variables, OI, the most important one, reinforces income bias around 20-30 percent of the partial income elasticity in participation. The biasing effect of OI is reversed by high political participation from rural residents who benefit inadequately from economic development. Another factor making the Japanese structure of participation income-neutral is that highly educated citizens do not necessarily participate more in politics than those who are less well educated; in fact, they even withdraw from some forms of political activities.

What are the implications of this Japanese pattern of political participation for income distribution and economic development? Clearly, the effect of the place of residence on the extent of income bias in participation is important in the analysis of the role of political participation in achieving equitable growth. Previous findings indicate that increased income inequality accompanying economic development is closely related to the widening of intersectoral inequality between the rural and urban sectors.[17] If there is a negative relationship between urbanism and the rate of participation (implying that rural citizens participate more in politics in this stage of growth), we would expect a significant redistribution of income from the urban sector to the rural sector through the political system. Consequently, rural bias in participation overshadows the widening of intersectoral inequality, resulting in equitable growth. This is the main hypothesis to be tested in the next section.

[16] Bradley M. Richardson, "Urbanization and Political Participation: The Case of Japan," *American Political Science Review* 67 (June 1973), 433-52; Joji Watanuki, *Politics in Postwar Japanese Society* (Tokyo: University of Tokyo Press, 1977), 65-76.

[17] Simon Kuznets, "Quantitative Aspects of Economic Growth of Nations: Part VII, The Distribution of Income by Size," *Economic Development and Cultural Change* 11 (January 1963), Part 2.

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## The Effect of Rural Bias in Participation on the Pattern of Income Distribution

Available theories and empirical evidence suggest that the relationship between economic development and income inequality is bell-shaped.[18] In the growing stage of economies, the intersectoral inequality between the urban (modern) sector and the rural (traditional) sector becomes wider. Consequently, overall income inequality widens. However, as economies continue to grow, income inequality is narrowed because (1) income inequality within the urban sector narrows and (2) intersectoral inequality between the urban and rural sectors narrows.

In their empirical study, Adelman and Morris conclude that the increase in GNP per capita tends to widen income inequality at the lower level of development while economic growth narrows inequality at the higher level of economic development. Thus the relationship is nonlinear.[19] The hypothesis that the relationship between economic development and income inequality is bell-shaped seems plausible, but the question is whether this pattern continues indefinitely. Empirical investigations of the relationship over time in the United States suggest that there is a limit to the extent to which economic development influences the pattern of income distribution.[20]

On the basis of the above theories and findings, we expect the functional form of the relationship between economic development and income inequality to be nonlinear: in the early stage of developing economies, the relationship is convex, whereas for substantially developed economies, it gradually becomes horizontal, as shown by curve A in Figure 2.

We expect the path of Japanese income distribution accompanying economic development to deviate from this general pattern and to appear as line B. The main difference is that line B skips the bump in an income inequality/economic development relationship. Since the positively sloped portion of curve A is closely related to the widening of intersectoral inequality following rapid economic growth, a significant redistribution of income from the urban sector to the agricultural sector through the political system should reverse the natural tendency of the widening of

[18] *Ibid.*; also see Jerry Cromwell, "Income Inequality, Discrimination, and Uneven Capitalist Development," Ph.D. diss. (Harvard University, 1974); Adelman and Morris (fn. 3).

[19] Adelman and Morris (fn. 3), 165.

[20] T. Paul Schultz, "Secular Trends and Cyclical Behavior of Income Distribution in the United States: 1944-1965," in Lee Soltow, ed., *Six Papers on the Size Distribution of Wealth and Income* (New York: N.B.E.R., 1969); Barry R. Chiswick and Jacob Mincer, "Time-Series Changes in Personal Income Inequality in the United States from 1939, with Projections to 1985," *Journal of Political Economy* 80 (May/June 1972), 34-73.

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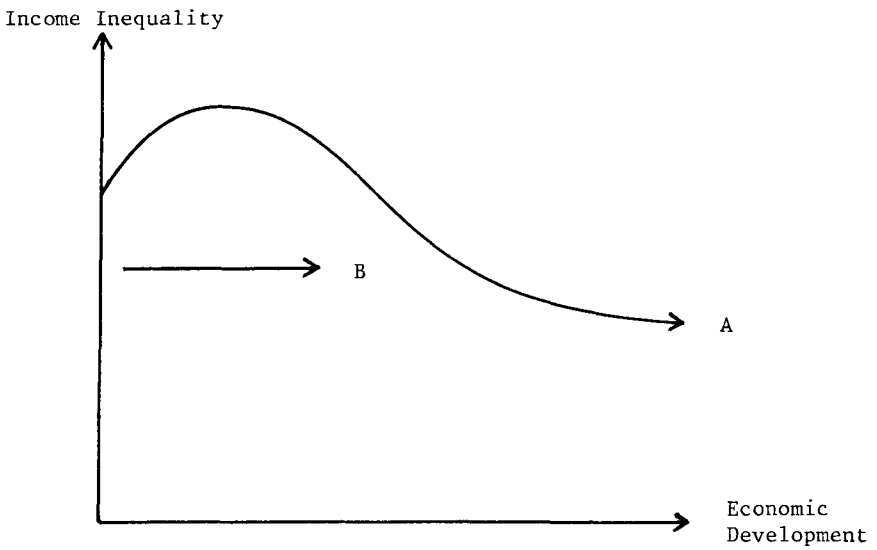
FIGURE 2
HYPOTHETICAL RELATIONSHIP BETWEEN INCOME INEQUALITY AND ECONOMIC DEVELOPMENT




intersectoral inequality. The above hypothesis is shown in the following causal diagram.

Rural bias in political participation → Political redistribution of income from the urban to rural sectors → Equitable growth

We will test the above explanation of causality in reverse order by asking the following questions:

1. Did the Japanese inequality/economic development line skip the bump (i.e., achieve equitable growth)?
2. Was skipping the bump a result of narrowing intersectoral inequality?
3. Was the narrowing of intersectoral inequality a result of the redistribution of income from nonagricultural to agricultural sectors through the political system?
4. Do farmers participate more in politics than do other occupational groups?

Figure 3 answers the first question. The x- and y-axes of the figure indicate the extent of income inequality measured by GNP per capita and the Gini coefficient, respectively. Scales differ for the global pattern and Japan, since the former is an aggregate. The scale under the x-axis

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is for the global pattern; the one above it is for Japan. The global curve is obtained by computing the Gini data from 62 countries (excluding the communist states).[21] The global trend line shows a relatively small income inequality in the very early stage of development; it peaks with a per capita income of $200-400. The curve then begins to decline, at first rapidly, then gradually. The Japanese inequality/economic development curve does not skip the bump completely.[22] There is a clear tendency toward a widening of income inequality between 1956 and 1962. After 1962, however, this tendency is reversed quickly. As a consequence, there is only a small bump in the inequality/economic relationship in Japan as compared to the global pattern.

Figure 3 also illustrates that the rate of economic development is very high during this period. Between 1956 and 1971, the increase in per capita income in Japan is almost tenfold in nominal terms. Previous studies have shown that the relationship between economic development and income inequality is negative in the long run, but that the rate of growth is positively related to income inequality.[23] This generalization is partly true and partly false for Japan: the progression of income inequality in the early period of development is reversed even though the country was still experiencing high economic growth. What, then, are the sources of such a pattern of income distribution? If our hypothesis is correct, the narrowing of intersectoral inequality should be an important source of overall income equality.

To test the above hypothesis, we divided the Japanese population into two sectors: agricultural and nonagricultural. Figure 4 shows that there is a basic lack of variability across time in income inequality for each sector.

In the nonagricultural sector, we do find a minor decline in income inequality from a Gini ratio of 0.33 in 1959 to one of 0.27 in 1969. Mizoguchi suggests several reasons: (1) diminishing profit differentials between big plants and small plants; (2) a shortage of young workers in the labor force, increasing the incomes of young people; and (3) diminishing earnings inequality among cities of different sizes.[24] The

[21] The income distribution data and GNP compiled by Shail Jain, *Size Distribution of Income* (Washington, D.C.: World Bank, 1975), include a relatively large number of countries in both the communist and noncommunist countries. We used income distribution data based on distribution of family income sampled from the national population between 1960 and 1970.

[22] The Japanese income distribution data used here are from Toshiyuki Mizoguchi, "Sengo Nihon no Shotoku-bunpu to Shisan-bunpu" [The Distribution of Income and Wealth in Postwar Japan], *Keizai-Kenkyu* 25 (October 1974), 360.

[23] Adelman and Morris (fn. 3); Jackman (fn. 3).

[24] Mizoguchi (fn. 22), 355.

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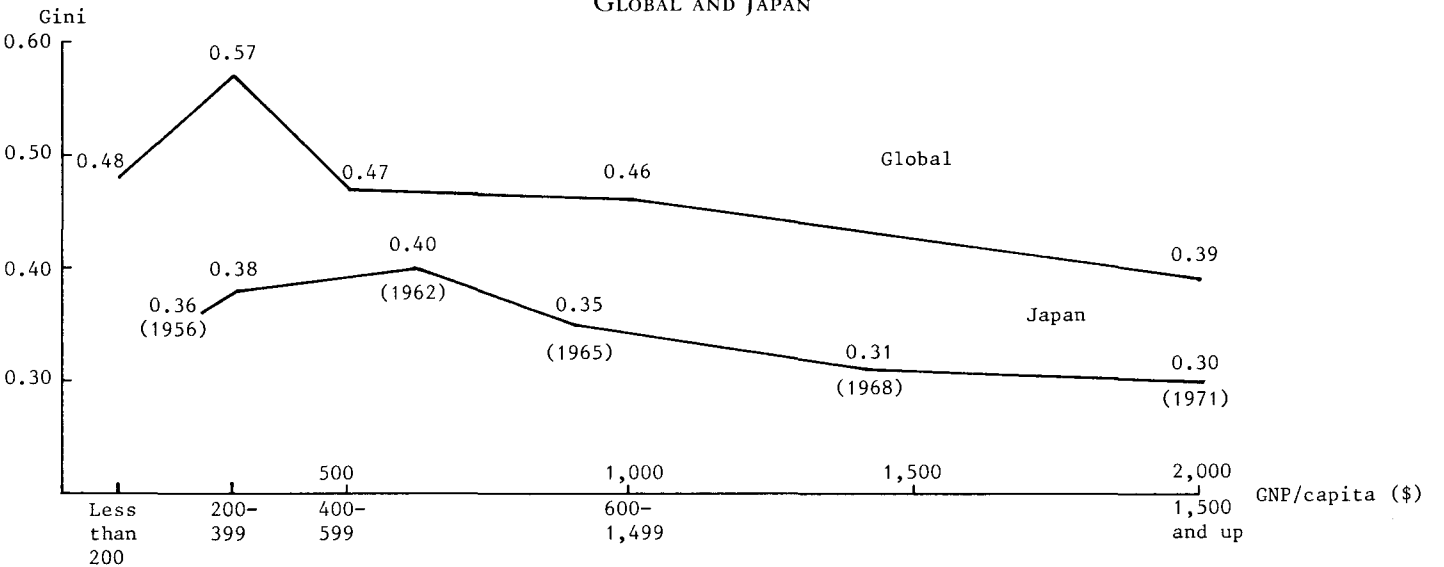
## FIGURE 3

### THE RELATIONSHIP BETWEEN GINI RATIOS AND THE LEVEL OF ECONOMIC DEVELOPMENT, GLOBAL AND JAPAN




Note: The Gini ratio, ranging from 0 to 1, measures the degree of income concentration in the population. The greater the ratio, the greater the inequality.
Source: Jain (fn. 21), for global data; Mizoguchi (fn. 22), 360, for Japanese data.

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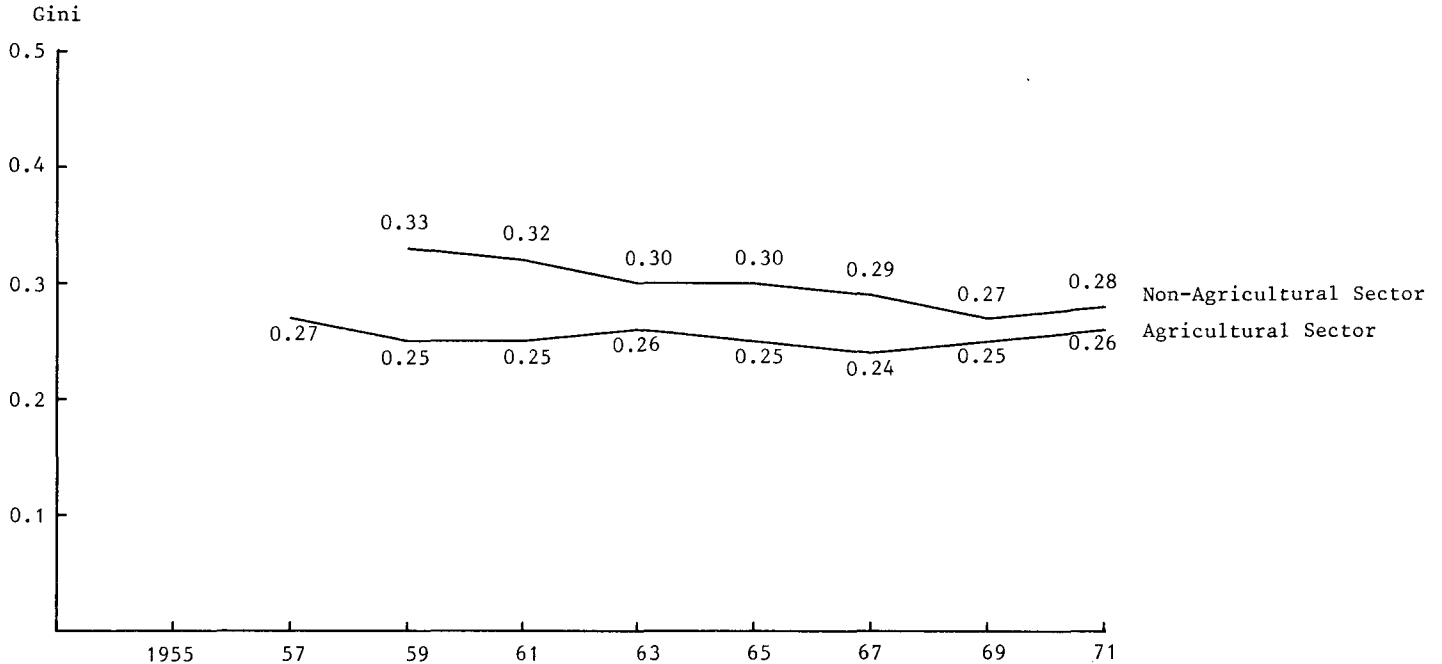
FIGURE 4
TRENDS OF INCOME DISTRIBUTION
IN THE AGRICULTURAL AND NON-AGRICULTURAL SECTORS,
JAPAN, 1957-1971




Source: Keizai Kikaku Cho, *Shotoku oyobi Shisan Bunpai no Genjo to Mondai-ten* [Status and Problems of Income and Wealth Distribution] (Tokyo, 1975), 203, 210.

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degree of variation and the direction of change of income inequality within the agricultural sector are not significant or clear.

The extent of the decline in income inequality within each sector does not explain the decline of overall inequality from 1962 to 1971. The overall decline in the Gini ratio is 0.1, while for the nonagricultural sector alone, it is 0.05. An increasing urban population during this period should increase overall inequality, because inequality in the urban sector is greater than in the rural sector. Moreover, neither the change in income inequality within the nonagricultural sector nor the change within the agricultural sector corresponds with the progression of overall income inequality between the late 1950s and the early 1960s.

Turning to an analysis of the intersectoral inequality between the agricultural and nonagricultural sectors, we should expect a change in intersectoral inequality that corresponds to the change in overall income inequality.

TABLE 3
INTERSECTORAL DIFFERENCES IN THE GROWTH OF MEAN HOUSEHOLD INCOME

| | *Workers' Households* | *Farmers' Households* | *Intersectoral Differences* |
|---|---|---|---|
| 1955 | $ 973 | $ 995 | $ − 22 |
| 1957 | 1,089 | 946 | 143 |
| 1959 | 1,229 | 1,035 | 194 |
| 1961 | 1,504 | 1,276 | 228 |
| 1963 | 1,776 | 1,374 | 402 |
| 1965 | 2,171 | 1,832 | 339 |
| 1967 | 2,624 | 2,951 | 33 |
| 1969 | 3,255 | 3,127 | 120 |
| 1971 | 4,152 | 4,056 | 96 |
| 1973 | 5,228 | 5,985 | − 757 |
| 1975 | 7,871 | 8,844 | − 973 |

Note: Unit is U.S. dollar based on the exchange rate in 1965 ($1.00 = 360 Yen).
Source: Sorifu Tokei Kyoku (Bureau of Statistics, Office of the Prime Minister of Japan), *Kakei Chosa* [Family Income and Expenditure Survey] (Tokyo: 1977), 7.

Table 3 illustrates the intersectoral differences in the growth of household income between workers' households and farmers' households. In 1955, ten years after the end of the war, the income of workers' households was lower than the income of farmers' households, reflecting the fact that the industrial production bases in the urban areas had not fully

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recovered from the total destruction of the war, and the agricultural sector was more productive at that time. With the beginning of postwar economic development after 1955, the income of workers increased rapidly, overtaking that of farmers; as a result, the intersectoral inequality between workers' and farmers' households increased sharply. This progression of the intersectoral inequality between 1955 and 1963 corresponds with the progression of the overall inequality shown in Figure 3. After 1963, intersectoral inequality declined rapidly, corresponding to the diminishing of overall inequality. The expansion of farmers' income proceeded more rapidly than that of workers' income, with the former eventually outstripping the latter. What made the farmers' income increase so rapidly as to overshadow the widening of intersectoral inequality in the early stage of the postwar economic development?

One possible explanation for the rapid expansion of farm income is that agricultural productivity expanded and eventually outstripped that of wage earners. This explanation fails intuitively as well as empirically. Both the absolute level of productivity and the rate of growth in productivity of the manufacturing sector are higher than those of the agricultural sector in the postwar period.[25]

Another possible explanation is the increase in the number of part-time farmers. Economic growth caused an insufficient supply of labor, resulting in the mechanization of agriculture, and giving farmers an opportunity to work part-time in industry while continuing to farm. The large number of part-time farmers among the agricultural population partially explains the growth of farm income, which derives to a significant extent from the growth of wage income. However, it is still puzzling that the mean income of the agricultural sector has outstripped the mean income of the urban sector: in view of the part-time nature of nonfarm occupations taken by farmers, the growth in part-time wages should not exceed the growth in wages of full-time workers.

The most probable reason for the rapid increase in farm income is that, while the income from farming remains comparable to that of wage earners, farmers utilize the spare time available to them through the mechanization of agriculture to earn *extra* income from part-time employment. This is possible because the size of the average farm in Japan is only approximately 2.5 acres. The question then becomes, what makes the income from farming—with farmers spending little time on their small farms—comparable to wage income?

[25] Norin Tokei Kyokai, *Nogyo Hakuso* [Agricultural White Paper] (Tokyo, 1978), 29.

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The income of farmers has been affected by politics. In order to raise the income of farmers, the government has maintained high price supports for agricultural products and has heavily subsidized the improvements that have made the mechanization of agriculture possible. Donnelly describes the situation as follows:

> A major issue concerning Japanese economic policy in the 1960's involved the extraordinary rise in prices paid by the government to rice producers. Beginning in 1960 producer prices doubled in eight years from 10,405 yen to 20,640 yen per 150 kilograms of unpolished rice. This rise was far greater than that in price paid by consumers which were also officially determined.[26]

In addition, in order to improve the economic situation of farmers in the long run, it was necessary to adopt new technologies. Since small farmers in Japan had neither the capacity to finance large investments in agricultural machinery nor a way to make the land suitable for modern machinery, the government of Japan has subsidized them heavily. Table 4 illustrates the share of government spending that serves the farming population in Japan (the amount above any general services offered by other ministries of the government) and governmental expenditures on agricultural price support programs, as compared to those on defense and social welfare.

Table 4 demonstrates the following:

1 In 1955, 11 percent of the national expenditure was allocated to the agricultural sector. The fact that this ratio remained at the same high level throughout the 1950s and 1960s is particularly significant in view of the fact that the number of farm households declined drastically over the same period.
2. The expenditure on the agricultural sector is disproportionately high in comparison to defense spending; in 1973, the former was twice that of the latter.
3. The expenditure on social welfare programs has grown significantly since 1961. It is possible that this has contributed to the decline in overall inequality.
4. A significant portion of the government's total expenditure on agriculture is allocated to the price support program for rice and other agricultural commodities.

Table 5 shows governmental spending on the agricultural sector in Japan to be disproportionately high in comparison to the United States,

[26] Michael W. Donnelly, "Setting the Price of Rice: A Study of Political Decisionmaking," in T. J. Pempel, ed., *Policymaking in Contemporary Japan* (Ithaca, N.Y.: Cornell University Press, 1977), 146.

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TABLE 4

THE STRUCTURE OF GOVERNMENTAL EXPENDITURES IN JAPAN: 1955-1975 (Unit = 100 million yen)

| | *A* *Total Expenditure* | *B* *Agriculture* | | *B-1* *Agricultural Price Supports* | | *C* *Defense* | | *D* *Social Welfare* | |
|---|---|---|---|---|---|---|---|---|---|
| | *Amount* | *Amount* | *(B/A)* | *Amount* | *(B-1/A)* | *Amount* | *(C/A)* | *Amount* | *(D/A)* |
| 1955 | 10,133 | 1,113 | (.11) | — | | 1,347 | (.13) | 1,038 | (.10) |
| 1957 | 11,846 | 1,183 | (.10) | 150 | (.01) | 1,430 | (.12) | 1,161 | (.10) |
| 1959 | 15,121 | 1,512 | (.10) | 0 | (0) | 1,556 | (.10) | 1,480 | (.10) |
| 1961 | 20,074 | 2,296 | (.11) | 660 | (.03) | 1,835 | (.09) | 2,454 | (.12) |
| 1963 | 30,568 | 2,984 | (.10) | 740 | (.02) | 2,476 | (.08) | 3,891 | (.13) |
| 1965 | 37,447 | 3,981 | (.11) | 1,205 | (.03) | 3,054 | (.08) | 5,458 | (.15) |
| 1967 | 52,034 | 6,158 | (.12) | 2,415 | (.05) | 3,870 | (.07) | 7,396 | (.14) |
| 1969 | 69,309 | 8,244 | (.12) | 3,530 | (.05) | 4,949 | (.07) | 9,743 | (.14) |
| 1971 | 96,590 | 11,452 | (.12) | 2,601 | (.03) | 6,935 | (.07) | 13,418 | (.14) |
| 1973 | 152,726 | 18,709 | (.12) | 5,380 | (.04) | 9,790 | (.06) | 22,196 | (.15) |
| 1975 | 212,888 | 21,768 | (.10) | 7,520 | (.04) | 13,273 | (.06) | 39,269 | (.18) |

Source: Ōkurasho [The Ministry of Finance of Japan], *Kuni no Yosan* [The National Budget] (Tokyo: 1960, 1965, 1970, 1975).

TABLE 5

AGRICULTURAL EXPENDITURES AS A PERCENTAGE OF TOTAL GOVERNMENTAL EXPENDITURES IN MAJOR COUNTRIES 1971–1977

| | *1971* | *1974* | *1975* | *1976* | *1977* |
|---|---|---|---|---|---|
| Japan | 12.0 | 10.5 | 10.0 | 8.8 | 7.9 |
| United States | 2.0 | 0.8 | 0.5 | 0.7 | 1.4 |
| Britain | — | 4.0 | 3.2 | 2.1 | 1.5 |
| West Germany | 3.8 | 1.6 | 1.4 | 1.2 | 1.1 |
| France | 3.3 | 3.8 | 3.8 | 3.7 | 3.7 |

Source: Ōkurasho [Ministry of Finance], Zaisei no Genjō to Tenbō [The Status and Future of Public Finance], unpub. internal mimeo., 1980.

England, West Germany, and France. In the latter countries, agricultural support as a share of total spending amounts to between 2 and 4 percent of the national budget, whereas in Japan it is 8 to 12 percent. The evidence suggests that in Japan, a redistribution of income occurs from the nonagricultural to the agricultural sector through the political system.

Finally, we examine the proposition that political participation will be higher in the agricultural sector than in other occupational groups. Table 6 compares the rate of political participation among different occupational groups, again based on data collected by the Cross-National Program in Political Change. The participation score in the table was standardized so that the average score for the entire population is zero, and one standard deviation above and below the average is 100 and − 100, respectively. The figures in Table 6 are consistent with our hypothesis. The rate of participation of farmers in voting, campaign, and communal activities is approximately 0.1 to 0.2 standard deviations higher than the average. It is plausible that, because of this higher level of participation, farmers can apply strong pressures on the government and are able to communicate their preferences to governmental decision makers.

The analysis thus far has shown that high political participation from the rural sector has reversed a natural tendency toward widening inequality between the rural and urban sectors during the period of rapid growth. The question as yet unanswered is why high political participation by the rural sector in the growing stage did not undercut the rate of growth.

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TABLE 6
COMPARISON OF THE RATE OF POLITICAL PARTICIPATION AMONG OCCUPATIONAL GROUPS IN JAPAN

| | *Voting Activity* | *Campaign Activity* | *Communal Activity* | *N* |
|---|---|---|---|---|
| Service | −1.7 | −6.8 | −16.0 | 148 |
| Factories and Mines | −2.9 | −10.8 | −17.7 | 598 |
| Transportation and Communication | 0.9 | 9.3 | −12.4 | 127 |
| Agriculture | 9.1 | 21.0 | 13.9 | 529 |
| Sales | 3.6 | 1.0 | 3.8 | 186 |
| Clerical | −8.2 | −14.0 | −6.0 | 375 |
| Professional | 6.6 | −2.8 | 20.0 | 168 |
| Top Management and Administration | −2.6 | 16.3 | 35.4 | 100 |

Note: Participation score is standardized so that the average score for the entire population is zero, and one standard deviation above and below the average is 100 and −100.
Source: Survey data collected in 1966 by the Cross-National Program in Political and Social Change.

## TOWARD A NEW MODEL OF DEVELOPMENT: A MODEL OF SUPPORTIVE PARTICIPATION

Huntington and Nelson postulate technocratic and populist models for the later phase of a country's economic development. The technocratic model

> assumes that political participation must be held down, at least temporarily, in order to promote economic development, and that such development necessarily involves at least temporary increases in income inequality

as this

> widening gap between rich and poor, combined with governmental efforts to repress political participation, build[s] up stresses and pressures and lead[s] eventually to a "participation explosion"[27]

as illustrated in the diagram on page 331.[28]

[27] Huntington and Nelson (fn. 1), 23.
[28] *Ibid.*, 24.

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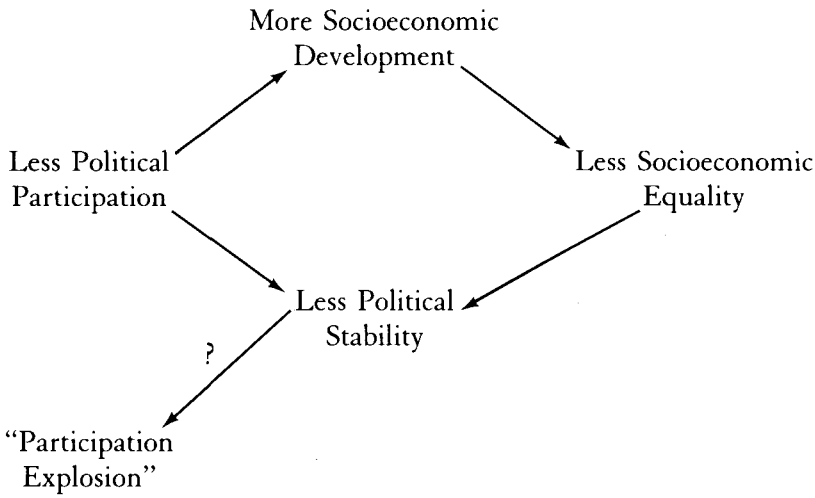
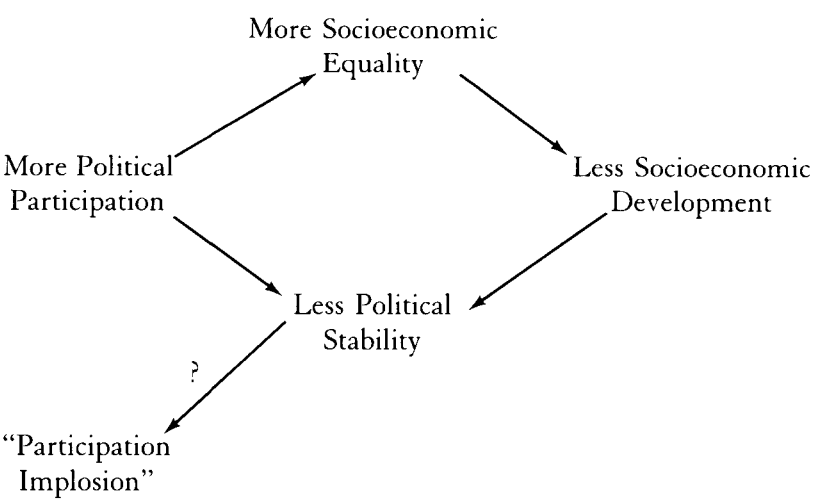



In the populist model of development,

> high and increasing levels of political participation go with expanding governmental benefits and welfare policies, increasing economic equality, and if necessary, relatively low rates of economic growth. The logic of this pattern of evolution leads toward increasing social conflict and the polarization of society, as more groups become participant and attempt to share in a stagnant, or only slowly growing, economic pie.

Eventually, this may lead to "participation implosion,"[29] as shown in the following diagram.[30]




[29] *Ibid.*, 23.

[30] *Ibid.*, 25.

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Japan has followed a developmental path that varies from the two suggested by Huntington and Nelson. In its development, Japan has not only traveled a path of equitable growth, but has done so very rapidly. How are we to reconcile equality, rapid growth, and broad-based political participation after the war? The answer seems to rest on the phenomenon of rural participation as supportive participation.

Several factors account for Japan's rapid economic development after World War II. Among the most important are (1) a high rate of savings, permitting rapid capital formation; (2) the high quality of the labor force, which is educated, motivated, and diligent; and (3) effective technology transformation. While these economic and social factors were important in achieving rapid economic development, political factors were no less so. Huntington and Nelson imply that a technocratic government is best suited to rapid economic growth because political elites need to make authoritative decisions in order to initiate and carry out effective economic policies. Moreover, stability and continuity in government are required to implement economic plans.

In Japan, this stability and continuity has been attributed to the dominance of the Liberal Democratic Party, which has been in control of the government literally since the Democratic Party and the Liberal Party merged to form the LDP in 1955. By their overwhelming support for the party, farmers have played a significant role in maintaining this dominance. More accurately, the LDP has depended heavily on farmers for votes and on businessmen for political contributions.

Table 7 presents data on party support by occupational groups during the period of rapid growth. It is clear from the table that farmers and top managers and administrators constitute the largest group of supporters of the LDP. Approximately 71 percent of those farmers who supported any party, and 75 percent of all managers and administrators, supported the LDP. This fact, combined with the high level of political participation of these groups, has made governmental decision makers extremely sensitive to the demands of farmers and economic elites.

Table 7 suggests an explanation of why substantial redistribution policies in Japan are compatible with rapid economic growth. Governmental policies favoring high economic growth (through industrial development) naturally meet the expectations of economic elites. But the achievement of high economic development requires a certain continuity in government and consistency in policy; the economic elites alone are not a sufficient base of support for the ruling party in Japan. Although the Liberal Democrats have managed to broaden their base of support considerably during the years of economic growth, it was the farmers

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TABLE 7

PARTY SUPPORT BY OCCUPATIONAL GROUPS IN JAPAN
(Percentages)

| | *Liberal Democrats* | *Socialists* | *Democratic Socialists* | *Communists* | *Komeito* | *N* |
|---|---|---|---|---|---|---|
| Service | 55.0 | 29.4 | 6.4 | 4.6 | 9.2 | 109 |
| Factories and Mines | 35.9 | 49.9 | 5.6 | 1.9 | 6.4 | 373 |
| Transportation and Communication | 27.8 | 59.7 | 8.3 | 1.4 | 2.8 | 72 |
| Agriculture | 70.8 | 23.5 | 3.1 | 0.5 | 2.0 | 391 |
| Sales | 63.3 | 28.8 | 5.5 | 0 | 5.5 | 128 |
| Clerical | 45.9 | 43.0 | 8.3 | 1.2 | 1.7 | 242 |
| Professional | 35.1 | 58.6 | 5.4 | 0.9 | 0 | 111 |
| Top Management and Administration | 74.6 | 16.4 | 4.5 | 1.5 | 3.0 | 67 |

Source: Survey data collected in 1966 by the Cross-National Program in Political and Social Change.

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who continued to back the party by a consistent and crucial margin. This is particularly true because—despite the rapid decrease in the farm population—rural areas remain overrepresented, and most electoral districts in Japan's lower house retain a small but critical agricultural component. In return for their support, farmers receive preferential treatment through the budgetary system, which substantially equalizes the intersectoral inequality that might otherwise result from rapid growth.

Supportive rural participation is also related to political culture. Almond and Verba maintain that democracy requires a certain balance between the power of political elites and the responsiveness of these elites to the people.[31] However, the promotion of economic development often requires a shift in balance in the direction of increased power to the elites in order for them to carry out effective economic policies. Since rural participation tends to be "passive, uninvolved, and deferential to elites,"[32] it does not undercut authoritative decision making by the elites. Rather, supportive rural participation, in the period shortly after Japan allowed broad-based political participation, softened the intensity of divisiveness and tension among social groups and the demands for radical change that accompanied economic development.

Moreover, supportive rural participation during the country's period of rapid growth was based to a significant degree on village solidarity that was institutionalized through various intermediate groups. As Watanuki has pointed out, the

> need for cooperation in the control of water for irrigation, common worship of village Shinto gods, common ties to the same Buddhist temple, concentrated physical location of houses in a narrow area, networks of extended family ties and other conjugal ties, and the administrative practice of utilizing the village as an administrative unit ... created a high degree of village solidarity.[33]

Although in the prewar and immediate postwar years local influentials played a dominant role in relating these local social networks of the village to political processes at higher levels, administrative amalgamations of local governmental units in the 1950s and early 1960s created the need for intermediate organizations on a broader scale. Agricultural cooperatives filled this role on one level; on another, conservative politicians organized constituency-wide individual support groups (*Koenkai*). These organizations have become the principal means for mobilizing rural voters and distributing governmental benefits. The intense

[31] Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, paperback ed., 1965), 1-35, 337-74.

[32] *Ibid.*, 344.

[33] Watanuki (fn. 16), 82.

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concern of rural voters with improvement of local conditions has made them particularly receptive to politicians from the party in power, who can respond with governmental largess.[34]

The pattern of combining supportive participation with substantial redistribution was further reinforced by the policy-making style developed in Japan since the 1870s: elite bureaucrats have dominated policy making, and parties have acted as agents of outsiders pressing claims against the bureaucracy. In contrast to the bureaucrats' efforts to concentrate resources, party politicians have had a strong redistributive orientation. Since 1955, the power elite has been composed of senior leaders of the LDP, senior bureaucrats, and representatives of big business who are interested in achieving rapid economic development.[35] Members of the Diet, on the other hand, have lobbied heavily with the bureaucrats to promote rural development (and, hence, redistribution) in order to win rural support for their reelection.

The success of rural voters and politicians in disseminating government funds to their rural districts is of course partly dependent on the availability of these funds. Economic growth and the subsequent increase in government revenues have made massive resources available for redistribution to the rural sector. According to Campbell, the average yearly growth in governmental revenues between 1954 and 1974 was 14.4 percent in constant prices; this increase is attributable to the progressive tax system which has allowed governmental revenues to grow more rapidly than GNP.[36] In 1955, total governmental expenditure amounted to barely one trillion yen; by 1975, it had increased to approximately 21 trillion yen. (See Table 4.) Since the ratio of agricultural expenditures to total governmental spending remained constant during this period while the number of farm households declined drastically,

[34] To please these supportive groups, the LDP government often uses the policy of giving subsidies to them. As a result, the share of governmental subsidies in the national budget is much larger in Japan than in other countries. For example, the subsidies in Japan's budget amount to around 32 percent whereas in England and France, they are 4 to 6 percent, in the United States 8 to 10 percent, and in West Germany 18 to 20 percent. Approximately 14 percent of the total is allocated to the agricultural sector in Japan; of 1,500 items, 474 go to the agricultural sector. See Michisada Hirose, *Hojokin to Seikento* [Subsidy and the Party in Power] (Tokyo: The Asahi, 1981), 76, 98. Hirose also discusses the mechanisms by which subsidies are apportioned.

[35] The triumvirate model is popular among political scientists in Japan. See Haruhiro Fukui, "Studies in Policymaking: A Review of the Literature," in Pempel (fn. 26), 23-59. See also Joji Watanuki, "Kodo Seicho to Keizai Taikokuka no Seiji Katei" [Political Process of Japan's High Economic Growth and her Emergence as an Economic Giant 1955–1977] *Nihon Seijigakkai Nenpo* [Annuals of the Japanese Political Science Association] (1977), 141-92.

[36] John Creighton Campbell, *Contemporary Japanese Budget Politics* (Berkeley: University of California Press, 1977), 3.

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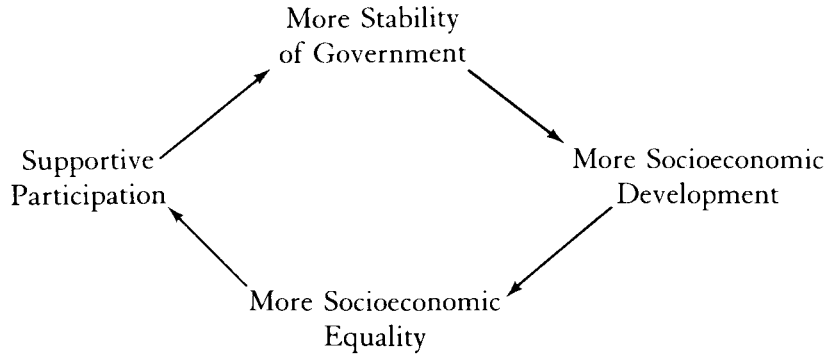
the absolute amount allocated to individual farmers increased substantially. This increase in agricultural spending through rapid economic development reinforced supportive participation. Hence, the Japanese pattern may be called a *model of supportive participation in development*, as summarized in the following diagram.




The dynamics of the model of supportive participation are summarized as follows:

1. The greater the supportive participation by less privileged groups, the more continuity and stability in government.
2. The greater the continuity and stability in government, the greater the economic development while other economic factors are held constant.
3. The greater the economic development, the greater the opportunity for redistributing income from the more privileged to the less privileged.
4. The greater the redistribution, the more supportive participation and stability in government.

It should be obvious, therefore, that the model of supportive participation is more stable than the two models suggested by Huntington and Nelson.

## Summary

This paper provides a basis for improving our understanding of postwar politics in Japan and the implications of the Japanese experience for models of development. In analyzing the structure of Japanese political participation and the relationship between the structure of participation and the pattern of income distribution accompanying economic development, we found that there was little income bias in political participation in Japan during its period of rapid growth. This low level

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may be attributed to the following factors: (1) the independent effect of income on participation was relatively low; (2) the independent effect of education on participation was at a minimum; and (3) there was a rural bias toward political participation that substantially corrected any possible income bias in participation. The rural bias in political participation was especially important for the determination of income distribution that accompanied economic development.

Economic development narrowed the size of income inequality in the long run, but this relationship was not linear. In the early stage of growth, there was a positive relationship between economic development and income inequality; this trend was reversed after peaking at a per capita income of $200-400. As the economy grew further, the inequality curve declined—at first rapidly, then more gradually. We assumed that this gradual decline was closely related to the widening of intersectoral inequality between the rural and the urban sector. (This was in fact true in Japan.) Examination of the Japanese case revealed a rural bias in political participation that resulted in a significant redistribution of income from the urban to the rural sector through the political system; this redistribution was of sufficient scale to overshadow the widening of the intersectoral inequality that had accompanied the earlier stage of growth.

High political participation from the rural sector which benefited inadequately from economic development did not undercut the rate of economic development because it was supportive participation. Stability and continuity of government is an important political source of growth; the rural sector in Japan enhanced this continuity and stability by its overwhelming support for the incumbent government, the Liberal Democratic Party. In return, farmers received preferential treatment through the budgetary system, which substantially equalized the intersectoral income distribution.

Although the present paper examines only the Japanese developmental path, it offers some implications for future studies in political development. Previous inquiries have suggested that broad-based participation in democracy is not necessarily positively related to economic equality when the level of economic development is controlled. The present findings suggest that high political participation by those who benefit inadequately from a growing economy has resulted in greater economic equality, at least in Japan. This finding tends to support Verba and Nie's proposition that socioeconomic neutrality in participation is positively related to economic equality, though not to expansion of democratic rights *per se*.

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This case study of Japan offers more than just a test of the hypothesis that political participation has a significant impact on the pattern of income distribution. Its primary implication for theories of development is that political participation by the have-nots is not *necessarily* a cost to economic development, as Huntington and Nelson have argued.

Is the Japanese case unique? This essay has emphasized domestic factors that govern the relationship among economic growth, income equality, and political participation. Analysts of comparative politics may wish to determine whether the domestic preconditions for fudging the trade-off are present in other societies. The present study has not emphasized international factors that may have affected the relationship across growth, equality, and participation. The U.S. occupation, aid programs, and asymmetric access to the American economy may have been important permissive factors during the period treated. Analysts of international politics may wish to determine whether present international circumstances permit developing nations to depart from the two paths identified by Nelson and Huntington. Nonetheless, participation by have-nots, rapid growth, and economic equality *can be* compatible with each other, as the Japanese case suggests.

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# INTELLIGENCE AND WARNING: Implications and Lessons of the Falkland Islands War

By GERALD W. HOPPLE*

WAS the Falkland Islands War preventable? Was it foreseeable? Almost inevitably, these two questions arise when a war erupts. The focus here is on political and strategic rather than tactical warning. What kinds of political preconceptions and strategic assumptions impinged on and shaped the decisions and actions of the two protagonists?

The potential lessons are manifold, but we must guard against premature or misapplied lessons. The basic theme of this essay is that the conflict was essentially not foreseeable. Whether it was preventable is another and more difficult question. In the short term, it was probably not avoidable, but long-term British defense policy and posture decisions as well as strategic assumptions on both sides led to what may have been an unnecessary war. In addition, the lessons of the war must be viewed in the context of the changing nature of the international system and in light of the prospects for crisis and conflict between allies and in the third world in the 1980s. Modern history demonstrates that conflict and war between formal allies are neither impossible nor especially rare; in this sense, the Falkland Islands War was not at all "deviant." These central issues will be explored in some detail, and more general implications for intelligence and warning will also be considered.

## The Central Issues

Major wars often begin with sudden attacks.[1] In the aftermath of such an armed conflict, a postmortem is often conducted and the conclusion is advanced that there was an "intelligence failure." Any national

* An earlier version of this article was presented to the Panel on the Military Lessons of the Falkland Islands War at the Annual Meeting of the International Studies Association, Mexico City, April 1983. I would like to thank Richard Betts of the Brookings Institution for his helpful comments. The conclusions and views expressed here are those of the author and do not necessarily reflect the policies of the Defense Intelligence Agency or the U.S. Government.

[1] Richard K. Betts, *Surprise Attack: Lessons for Defense Planning* (Washington, D.C.: Brookings Institution, 1982).

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intelligence bureaucracy includes one or more components tasked with tracking current affairs and trying to anticipate the threatening behavior of actual or potential adversaries. Therefore, if the other side attacks, the warning analyst has failed to warn.

This plausible generalization is quite frequently wrong. A pervasive fallacy—the hindsight bias—encourages us to assume that what we know after the fact could have been foreseen.[2] Nothing could be further from the truth; what Baruch Fischhoff calls the "silly certainty of hindsight" is an insidious manifestation of a common failure in human thinking and perceiving. It is only one of many cognitive psychological obstacles to effective and error-free inference and analysis.

There have been many case studies of warning or intelligence failure.[3] All of the cases, it should be emphasized, are potentially susceptible to the hindsight bias. They also tend to share other characteristics. For example, in retrospect we can see that many military attacks featured a variety of warning signals—embedded in a maze of both noise and deception. Furthermore, most occurred in the context of heightened tension that had been building up for some time. Often, there were several previous alerts that turned out to be false alarms. The Pearl Harbor case illustrates this pattern vividly; after June 1940, there were three distinct periods of very high tension and alerts in U.S.-Japanese relations (including November 1941, the month prior to the attack).

There has been some movement toward a "theory" of surprise and warning. Several explicitly comparative analyses have attempted to develop a portrait of prewar warning and response. One focuses on deception and the other, Betts's study, is more ambitious.[4]

The Falklands case seems to fit into the reconstructed general pattern. Like Pearl Harbor, the German attack on the Soviet Union in 1941, the outbreak of the Korean War in June 1950 and the Chinese intervention in November, and many other comparable attacks during World War

[2] Baruch Fischhoff, "Hindsight ≠ Foresight: The Effect of Outcome Knowledge on Judgment Under Certainty," *Journal of Experimental Psychology: Human Perception and Performance* 1 (No. 2, 1975), 288-99; Fischhoff, "The Silly Certainty of Hindsight," *Psychology Today* 8 (April 1975), 70-76; Fischhoff and Ruth Beyth, "'I Knew It Would Happen': Remembered Probabilities of Once-Future Things," *Organizational Behavior and Human Performance* 13 (February 1975), 1-16.

[3] The major case studies are cited in Steve Chan, "The Intelligence of Stupidity: Understanding Failures in Strategic Warning," *American Political Science Review* 73 (March 1979), 171-80. The warning aspect of the Falklands conflict is covered in the Rt. Hon. the Lord Franks, *Falkland Islands Review: Report of a Committee of Privy Counselors* (London: H. M. Stationery Office, January 1983).

[4] See, respectively, Betts (fn. 1); Barton Whaley, "Stratagem: Deception and Surprise in War" (Cambridge, Mass.: MIT, unpub., 1969). Roberta Wohlstetter's "Cuba and Pearl Harbor: Hindsight and Foresight," *Foreign Affairs* 43 (July 1965), 691-707, is a useful comparative case study.

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II and since, warning signals preceded the Argentine invasion. If we take the extended historical record as our frame of reference, the Anglo-Argentine conflict dates back in an active sense to 1965 (when the United Nations urged Britain and Argentina to negotiate) and ultimately to 1833 (when the British occupied the Falklands after a brief period of Argentine rule). There had been a number of previous invasion threats (particularly in 1977) and Argentina almost routinely indulged in sabre-rattling prior to yearly rounds of negotiations with the British. The attack itself was the culmination of several weeks of tension.

In a more fundamental sense, the genesis of the war can be attributed to an intellectual syndrome in which one or both sides relies on certain reassuring but misleading political and/or strategic assumptions. The preconceptions typically refer to "facts" about one's own capabilities and the other's capabilities, intentions, and risk calculations.

Perhaps the most dramatic recent illustration of the disastrous impact of such strategic assumptions was the Israeli belief in 1973 that Egypt would not attack until it had attained air superiority.[5] This central premise (the master belief in the Israeli strategic calculus) was accompanied by a secondary belief: Syria would not attack unless Egypt did. These two assumptions formed the Israeli "conception." Together, the two core assumptions operated as a strategic conceptual straitjacket, suppressing and biasing the interpretation of an incoming stream of contrary tactical indications.

To what extent did analogous political/strategic assumptions dominate the Argentine and British decision processes in the eight weeks preceding the invasion on April 2, 1982? This is the central analytical issue in the "postmortem" below.

## Strategic Warning: An Analytical Perspective

Most analysts of strategic warning agree that we can never eliminate surprise, banish deception, or develop a foolproof system of indicators. Even if consensus crystallizes around a forecast that X will attack, whether someone will launch war is only the most general analytical question. We must also confront the multiple and vexing issues of when, where, and how.[6]

Roberta Wohlstetter's masterful retrospective analysis of the Pearl Harbor case starts out with the fundamental and quite illuminating

[5] Janice Gross Stein, " 'Intelligence' and 'Stupidity' Reconsidered: Estimation and Decision in Israel, 1973," *Journal of Strategic Studies* 3 (September 1980), 155.

[6] Betts (fn. 1), 4.

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point that a Japanese attack was fully expected.[7] The other side was expectant in late 1941, but when, how, and even where were open questions. Siberia, Southeast Asia, and the Philippines were among the many potential Japanese targets.

Perhaps more than anyone else, Richard Betts has written about strategic warning cogently, systematically, and theoretically.[8] While work on intelligence and warning tends to be narrowly case-specific, Betts has pursued an explicitly comparative research approach. He has tried to extract relevant lessons and theoretical generalizations from past cases while remaining sensitive to the subtleties and ultimately unresolvable trade-offs involved in strategic warning analysis. He has surveyed and assessed the nature and costs and benefits of alternative intellectual and organizational solutions, recognizing that few of them offer permanent panaceas, almost all of them reduce one vulnerability at the cost of increasing another, and some intelligence failure is inevitable. Furthermore, what we think of as an intelligence failure is often political failure. In searching for culprits, Betts advises us to subpoena the decision maker as well as—and probably with more justification than—the intelligence analyst.

In his book *Surprise Attack*, Betts puts forth three propositions that define the parameters of his theoretical argument. First, the key cause of surprise is political failure, not intelligence failure. Secondly, sudden attacks occur in situations of prolonged tension; there have been no significant cases of "bolts from the blue" in the twentieth century. Thirdly, the victim's strategic assumptions trigger the critical miscalculations that produce surprise.

These three propositions constitute a useful point of departure for analyzing the Falklands war from the vantage point of warning and intelligence. The first is the most crucial from the perspective of the discussion here. Betts demonstrates conclusively that military attacks that start wars are often "surprises," but the defender has nevertheless received significant warnings during both the political and strategic phases of warning. Sometimes, there are literally hundreds of incidents and other indicators as well as several major alert false alarms.[9]

Is a surprise attack an intelligence failure or a political failure? To

[7] Roberta Wohlstetter, *Pearl Harbor: Warning and Decision* (Stanford, Calif.: Stanford University Press, 1962).

[8] In addition to his book (fn. 1), see "Analysis, War, and Decision: Why Intelligence Failures Are Inevitable," *World Politics* 31 (October 1978), 61-89.

[9] For example, there was a major invasion alert in South Korea on May 7, 1950, prior to the real North Korean attack in late June. During 1949, there had been 874 border violations in the Korean arena (Betts, fn. 1, 53-54).

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an extent, many failures of warning undoubtedly mix the two dimensions in an indissoluble blend. For most cases he examines, Betts exaggerates the extent to which genuine warnings emanated from the intelligence bureaucracy. Typically, intelligence analysts reflect the political premises and biases of decision makers—either because they have no choice (dissent from official preconceptions would be ridiculed or punished) or they share central beliefs with the policy community.

We should distinguish between political failure that is a direct outgrowth of a nation's defense posture (and other policy flaws) and pure analysis or technical warning failure. From the British viewpoint, fundamental policy errors seemed to contribute disproportionately to the outbreak of the war.[10] The case of the Soviet invasion of Czechoslovakia in 1968, in contrast, seems to have been more of an analysis failure (from the Western perspective). Technical warning failure, when the relevant information is simply not collected or processed quickly enough, is relatively rare with respect to major military attacks, although processing and distribution snafus are far from irrelevant. Essentially, policy failure probably arises most blatantly when the deterrence-provocation dilemma is incorrectly diagnosed. Using Robert Jervis' deterrence (Munich) and spiral (World War I) models,[11] we can classify World War II as a policy failure and World War I as an analysis failure.

Obviously, the typology of policy failure, analysis failure, and technical warning failure is simplistic and misleading. Very few real-world cases would fall unambiguously into one of the three categories. Even with hindsight, people will disagree vehemently about particular cases. But the ideal types of pure policy failure and pure analysis failure at least alert us to the fact that policy without intelligence can be the real cause of what is labeled an intelligence failure.[12]

The distinction between analysis failure and technical warning failure, which is also simplistic, is nevertheless of undeniable value for the purpose of assessing intelligence performance. Warning is not just sen-

[10] For example, see the article "The Inquest Into How It Began," *The Economist*, June 19-25, 1982, 12.

[11] Robert Jervis, *Perception and Misperception in International Politics* (Princeton, N.J.: Princeton University Press, 1976).

[12] The most extreme example is policy making without regular and meaningful inputs from intelligence. A much less extreme example of policy failure occurs when intelligence is "good" but policy turns out to be "bad." For example, intelligence may generate estimates that are fairly accurate and offer relatively valid warnings, but the decision maker may—for perfectly justifiable policy reasons—feel obligated to take action inconsistent with the inputs. Then, the disaster is a policy failure, not an intelligence failure. See Richard K. Betts, "Intelligence for Policymaking," in Gerald W. Hopple, Stephen J. Andriole, and Amos Freedy, eds., *National Security Crisis Forecasting and Management* (Boulder, Colo.: Westview, 1983).

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sors and hardware; it is also analysis. More generally, intelligence is not simply facts in the form of data; it is also analysis and interpretation. Intelligence failures typically lead to injunctions about the need to collect more data. This is a myth; the problem is not generally the need for more data (analysts are often inundated with data), but the absolutely indispensable necessity for more and better analysis.

We need to emphasize what analysis can contribute to minimizing warning failure. Consumers want answers; producers, however, can often contribute most by posing questions.[13] In addition to asking the most relevant and difficult questions, warning intelligence should highlight the critical gaps in the knowledge of decision makers, illuminate the key uncertainties, clarify the ambiguities to the extent possible, and provoke debate whenever necessary.

The second proposition, that there have been no real "bolts from the blue" in the case of modern wars, is fundamentally valid and frequently ignored. As the next section will show, the Falklands case fits this pattern. The problem for the intelligence analyst, however, is twofold. One is the hindsight bias: Can we ever distinguish between the X case when war does occur and the few or many earlier Xs when tension rose, indicators were fully consistent with a reading of "significant danger of hostilities," but peace prevailed? The second is the challenge of attempting to analyze and forecast improbable but highly consequential events. On any given day, there are many potential crises that could escalate into wars. There have been thirty long-term bilateral conflict situations in Latin America alone since 1945, many of which are still active.[14] Coupled with the hindsight bias, the methodological difficulties of trying to explain and forecast rare but catastrophic events makes the job of the warning analyst more than challenging.

The third proposition identified above is that surprise attack is linked intimately with the victim's strategic assumptions. In one sense, this proposition is simultaneously unobjectionable and patently obvious. The attacker exploits the opponent's strategic premises; aside from instances in which the capability differential is extreme, if such assumptions did not exist, there would be nothing to exploit and no basis for launching a successful surprise attack. But in a more basic sense, strategic assumptions often emerge as genuine causal forces in a nontrivial way. When strategic assumptions account for surprise attack, they do so as

[13] Betts (fn. 8), 88.

[14] Wolf Grabendorff, "Interstate Conflict Behavior and Regional Potential for Conflict in Latin America," *Journal of Interamerican Studies and World Affairs* 24 (August 1982), 267-94.

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necessary (if not sufficient) determinants. The Israelis *assumed* that Egypt would not attack unless it had achieved air superiority, which it did not have in 1973 and was not expected to have until 1975. Stalin *assumed* in 1941 that Hitler would present an ultimatum before attacking. The West *assumed* that a North Korean attack in 1950 would inevitably be an adjunct to a more general war.

Strategic assumptions are almost invariably plausible (at least before the fact). They are also reinforced by the other side's active deception. Strategic assumptions may refer to capabilities or intentions (or both); they may concern basic defense postures and often reflect sensible but ultimately wrong recipes for preventing war. For example, Stalin tried to deter Hitler by avoiding provocation. In other cases, warning stimuli are simply rejected because they do not conform to the prevailing belief system.

People naturally become wedded to their basic beliefs and vigorously resist their elimination. This unwillingness to look at evidence in the light of alternative beliefs leads to warning disasters. The situation becomes even more problematic when, as is often the case, previous false alarms dull sensitivity and leave the strategic assumptions intact.

## The Falklands War: the warning context

The Falklands conflict began on April 2, 1982.[15] Although the immediate pre-crisis period spanned only the preceding month, Argentina had consistently demonstrated the seriousness of its purpose and shown its willingness to consider a resort to force since 1945.

In 1965 the United Nations General Assembly invited Britain and Argentina to negotiate for a peaceful solution to the dispute. There have been many rounds of negotiation since 1965, with the period since 1976 being characterized by a more determined effort, especially on the part of the British, to achieve a negotiated settlement.

The Falklands dispute has had many periods of relative quiet since the serious reassertion of Argentine sovereignty in the 1880s. Occasional crises, such as in 1927–1928, 1933, 1966, and 1976, have interrupted the sometimes long periods of quiescence.

[15] The historical background is provided in: Franks (fn. 3); Peter J. Beck, "Cooperative Confrontation in the Falkland Islands Dispute: The Anglo-Argentine Search for a Way Forward, 1968–1981," *Journal of Interamerican Studies and World Affairs* 24 (February 1982), 37-58; Peter Calvert, *The Falklands Crisis: The Rights and the Wrongs* (New York: St. Martin's Press, 1982); and Lawrence Freedman, "The War of the Falkland Islands, 1982," *Foreign Affairs* 61 (Fall 1982), 196-210.

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There was a particularly serious invasion threat in late 1977. Relations between Argentina and Britain were very strained, a British ship had been fired upon, and fuel supplies to the islands (which had become the province of Argentina several years earlier) had been cut off. Intelligence reports highlighted both Belize and the Falklands as potential flash-points. The British secretly sent a submarine as a precaution. The ensuing negotiations progressed well enough for the threat of invasion to subside. The false alarm is particularly important in the light of the events and interpretations of March 1982 because the Foreign Office had cried wolf once and had thereby weakened its case for the next time around.[16]

Since 1976, frequent Anglo-Argentine talks have occurred in New York, Lima, and Geneva. Exploratory negotiations in New York in April 1980 provided the background for a British policy initiative in November of that year and facilitated further talks during February 1981. The initiative constituted a significant breakthrough since the British publicly proclaimed their willingness to consider a solution that would involve Argentine sovereignty.

The February talks, scheduled for resumption in December 1981, were postponed by the fall of General Viola as head of the junta and his replacement by General Galtieri. The negotiations were held in February 1982 in New York.[17] On February 26, the two sides agreed to a compromise for a negotiating commission that would meet on a regular basis and the talks ended with a communiqué which referred to the "positive and cordial atmosphere." The war itself started a little over a month afterwards, with a successful Argentine invasion on April 2. Ten weeks later, Argentina surrendered to the British and conflict ended.

What went wrong? What happened between February 26 and April 2? Initially, the Argentine junta virtually repudiated its negotiator. War fever intensified in the press. The British representative to the New York talks, Richard Luce, conveyed his concern about the reaction in Buenos Aires to Lord Carrington. However, neither Luce nor Carrington felt that the situation warranted a formal request to send a tripwire or deterrent force. Indeed, in June 1981 a decision had been made to scrap the ice-patrol ship H.M.S. *Endurance*, the only regular British naval presence in the South Atlantic.[18]

[16] The Franks report (fn. 3, 87) points out that, at the time, the most serious risk was action against British shipping. The two frigates and the submarine were sent prior to the December 1977 talks, when it was believed that a breakdown in the negotiations was very possible. The circumstances surrounding the February 1982 talks were not analogous.

[17] Argentine press comment leading up to the New York talks stressed that a resort to military means was very possible during 1982. This was emphasized in *La Prensa* on January 24, 1982 and February 7, 1982 as well as in other newspapers and journals.

[18] At the time, the Foreign Office warned that this decision "could well be misread in Buenos Aires. This [action] left a garrison of some 70 Royal Marines to deter Argentina

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Two factors probably accounted for the failure to request a deterrent force.[19] One was budgetary, a climate of "total obsession with money," with the defense ministry "under perpetual siege."[20] The second was the absence of intelligence data that would have supported such an action. Apparently, neither the relevant Foreign Office analysts nor the Joint Intelligence Committee, an institutionally separate group within the Cabinet Office, produced assessments that would have justified a deterrent force decision.

Intelligence sources have since asserted that their raw material was significantly more alarmist than the assessment of it that went to the ministers. The assessments minimized the probability of an invasion, drawing on

> the old-hat nature of the threats, General Galtieri's preoccupation with domestic issues, improved relations between his regime and Europe and America, the "cordiality" of the New York talks and their one-year deadline. This was used to explain even the statement issued in Buenos Aires on March 3rd that the regime was about to "seek other means" of regaining the Falklands. This statement itself suggests the junta had not yet made any decision to invade—or it would surely not have issued it.[21]

*The Economist* concludes that a recommendation for a precautionary expedition would have required considerably more than circumstantial evidence. This was not available until March 29, the Monday before the invasion. By then, warning indicators had proliferated. The British reacted by sending a submarine plus support ships from the Mediterranean to the South Atlantic. By the time Prime Minister Thatcher called the first crisis meeting on Wednesday evening, deterrence by verbiage alone was clearly out of the question.

In contrast to many wars, the active prewar crisis period was unusually abbreviated. Furthermore, the British and Americans have since concluded that the junta had not made a definite decision to invade even on Monday. It was still an active option, but no final decision had been reached.[22]

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from attempting to retake the Falkland Islands by force" (Freedman, fn. 15, 198). See also Calvert (fn. 15), 66. Several Argentine newspapers featured versions of an article in the *Daily Telegraph* regarding this decision, with an emphasis on the theme that Britain was "abandoning the protection of the Falkland Islands" (Franks, fn. 3, 34).

[19] "Falkland Islands: The Origins of a War," *The Economist*, June 19-25, 1982, 36.

[20] *Ibid.*, 38.

[21] *Ibid.* The Franks report (fn. 3) discusses the intelligence assessments, but not the raw data.

[22] Admiral Jorge Anaya, the chief of the Argentine navy and the architect of the invasion, was actively lobbying for such an option. The invasion may have been triggered by the incident on March 19, when some Argentine scrap metal merchants raised the Argentine flag on the dependency of South Georgia. The *Endurance*, which was already scheduled to be withdrawn from the area, was sent with a detachment of 21 Marines from Port Stanley

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As in so many other crises of this nature, the victim was at least somewhat distracted by other issues. The foreign secretary's trip to Israel and another battle about the European Communities' budget both diverted attention from the Falklands.[23] In October 1973, the Israelis had been similarly distracted by a P.L.O. attack on Russian Jewish emigrants in Vienna. In November and December 1941, the war in Europe and other potential Japanese targets had preoccupied U.S. decision makers. In addition to false alarms, then, which reduce sensitivity through the false alarm effect, distraction militates against warning by diffusing attention.

Strategic assumptions played a role on both sides. The British assumed that Argentina would not try to take by force what it could not achieve by negotiations. This assumption was reinforced by the length of the dispute, the earlier false alarms, and the consistently strident Argentine rhetoric. Verbally, Argentina had repeatedly threatened invasion; this rhetoric came to be perceived as meaningless diplomatic posturing (and also as a flourish for domestic consumption). If anything, General Galtieri, with his Anglophile foreign minister, less draconian human rights policy, total neglect of the Falklands issue in his initial speeches, and decision to combat inflation by cutting defense spending (with the navy sustaining the largest reduction) seemed to offer a respite rather than a threat.

Another analytical flaw in the British perception of the situation was the serious underestimation of the strength of Argentina's feelings about the Falklands.[24] The British pursued a policy in which they attempted to substitute bluff for a credible defense posture vis-à-vis Argentina. The reason for this (and the consistent roadblock to a mutually acceptable solution of the conflict all along) was the feelings of the islanders, who were avowedly British in attitude and psychological identification and equally adamant in their opposition to any form of Argentine rule. The Foreign Office was further straitjacketed by Parliament, which backed the islanders without reservation. By early 1982, the British "could offer neither compromise to Argentina nor a credible long-term commitment

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to South Georgia. There has been speculation that this incident occurred with the knowledge and possible involvement of Anaya ("Falkland Islands," fn. 19, 43; Freedman, fn. 15, 199). Galtieri had pledged privately to take control of the Falklands by the time of the highly symbolic 150th anniversary of Britain's occupation (that is, by January 3, 1983) and the junta had made plans prior to the February talks to prepare for an effective occupation (Calvert, fn. 15, 56). See also Franks (fn. 3).

[23] Freedman (fn. 15), 200. Well into 1981, Britain had been preoccupied with another potential flashpoint in Latin America: the Guatemala-Belize crisis (Calvert, fn. 15, 68-69).

[24] "Falkland Islands" (fn. 19), 35; Jeffrey Record, "The Falklands War," *Washington Quarterly* 5 (Autumn 1982), 45.

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to the Falkland Islands. The only negotiating posture left was prevarication."[25] Bluff and stalling were hardly ideal negotiating postures for preventing a blowup when the other side felt as intensely as did Argentina.

Intensity is a far from infallible but nevertheless very useful guide to intentions and their link with behavior. Prior to the invasion, Argentina clearly felt much more intensely than the British. Although the Argentines may have thought that the British lacked the capability to retake the islands militarily,[26] the sheer intensity of their grievance—especially in comparison to Britain's commitment, which was reluctant, halfhearted, and ambivalent—undoubtedly played a role in the junta's final decision. The fact that the 150th anniversary of the British seizure would come in January 1983 must have added a powerful symbolic variable to the Argentine emotional equation.

Deterrence, defense, and escalation through miscalculation are three intertwined strategic concepts.[27] In her analysis of Israeli intelligence and decision making in the prewar environment in 1973, Stein explores the assumptions that govern the assessment and interpretation of information. As she notes, strategic arguments about deterrence, defense, and miscalculated escalation revolve around five distinct factors and their interrelationships:

— Evaluation of the interests at stake
— Assessment of the challenge to be deterred
— Examination of the adversary's intentions and calculations of the options available
— Consideration of the credibility of the commitment to respond
— Discussion of the appropriate response to the failure of deterrence

Based on the available evidence, it can be concluded that the quality of the British strategic concept was abysmal.[28] As Stein points out, the basic problem is not the use of some kind of organizing concept, which is absolutely necessary for both analysis and decision making.[29] The real issue is the concept's logical coherence and completeness, its relationship to other concepts in the larger systems of beliefs, and the way in which the concept is used. If, as Stein concludes, the Israeli concept in 1973 was incompletely articulated and this led to poor problem diagnosis and

[25] Freedman (fn. 15), 198. See also Franks (fn. 3), 79.
[26] Freedman (fn. 15), 199. See also Franks (fn. 3), 77.
[27] Stein (fn. 5), 152.
[28] According to Franks (fn. 3, 80), there were no detailed military contingency plans. Earlier plans had featured a "concept of operations," but were not detailed contingency plans (*ibid.*, 31-32).
[29] Stein (fn. 5), 152.

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suboptimal consideration of options (as well as aggravating biased information processing), then from this perspective the Argentine attack was perfectly understandable since, for all practical purposes, the British did not have a meaningful or workable strategic concept.

Why was this so? The easy answer is because the basic British assumption was that Argentina would not go to war. But this is the kind of assumption, both crude and derived from wishful thinking, that suggests a decidedly epiphenomenal character. A higher order assumption—or set of assumptions—beyond the Anglo-Argentine strategic context may be discerned. This refers to the fundamental British defense strategy and posture with respect to the conflict and, even more primordially, to the shopworn but valid adage that a nation must be sure that its "political objectives can be achieved with the available military forces and strategy."[30]

The British refused to cede the Falklands or make a real defense commitment to them. The latter would have required a decision that would have been perceived as politically damaging (if not suicidal) and economically unthinkable. But by early March, it was too late anyway; a decision to send a deterrent force at that time would not have sufficed.[31] An attack could have been deterred only by an earlier and substantial military commitment in the South Atlantic and preferably on the islands.

From the British perspective, the Falklands war is an almost classic decision disaster or policy failure. There were also analytical failures lockstitched into the fabric of the fiasco, but they were the kind of flaws that fall almost exclusively in the province of policy makers (e.g., establishing and assessing one's own detailed defense-deterrence strategy in a conflict arena).

Surprise attacks can be made *least* likely by overinsurance in force levels or very high levels of readiness; both are extremely expensive and neither is palatable.[32] However, British deterrence would have required neither overinsurance nor an unacceptably high readiness level. A credible deterrent force might have been sufficient and, if an extremely

[30] Michael Moodie, "Six Months of Conflict," *Washington Quarterly* 5 (Autumn 1982), 32. See also: Freedman (fn. 15), 198; Sir James Cable "The Falklands Conflict," *U.S. Naval Institute Proceedings* 108 (September 1982), 72; Admiral Thomas H. Moorer and Alvin J. Cottrell, "In the Wake of the Falklands Battle," *Strategic Review* 10 (Summer 1982), 27; James D. Hessman, "The Lessons of the Falklands," *Sea Power* 25 (July 1982), 16.

[31] In fact, at that point, a deterrent force would have ignited a preemptive Argentine attack (forcing them to advance their timetable slightly). If it had been too small to be noticed or had been sent secretly (as occurred in response to the 1977 invasion scare), it would not have prevented the desperate junta from invading. See "Falkland Islands" (n. 19), 43. See also Franks (fn. 3), 87.

[32] Betts (fn. 1), 309-10.

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improbable attack had occurred, would have greatly facilitated the defense of the islands.

Deterrence may be provocative in certain contexts. The deterrence-miscalculated escalation trade-off is particularly relevant to the analysis of certain defense situations. However, the Falklands situation was probably strongly analogous to the Korean arena today, where the danger is insufficient deterrence of a North Korean attack, not provocation.[33]

The argument above assumes a rational opponent (rational in the sense of weighing comparative capabilities and trying to maximize one's expected utility). We have very little knowledge of the process that led to Argentina's decision to invade. Extensive evidence indicates rather clearly, however, that inaccurate strategic assumptions shaped the junta's decision as much as equally wrong assumptions accounted for the British failure to anticipate war. If anything, the premise in Buenos Aires that the other side would not fight was held more strongly and perhaps with considerably more justification (based on signals and indications) than the equivalent assumption in London that Argentina simply would not go to war.

Argentina was reacting to the many British signals that they would neither make a long-term military commitment to the Falklands nor resolve the issue diplomatically. The decision to withdraw the *Endurance* from the area, which was postponed only because of the strange and semicomical South Georgia incident on March 19, had conveyed a significant message to Foreign Minister Costa Mendes. Other verbal and physical signals and indications emanated from London and were undoubtedly picked up in Buenos Aires.

The timing of the invasion shows a lack of concern for minimizing Britain's ability to react, underlining the plausibility of the argument that the junta assumed that Britain would not go to war (or would have satisfied itself with face-saving but essentially meaningless harassment).[34] Much of the British fleet was back home for Easter when the Argentines invaded, a fact that later facilitated the very rapid assembly of a powerful British task force. If Argentina had waited for only two more months, the fleet would have been dispersed (with a group of warships as far off as the Indian Ocean).

If Argentina had been willing to wait for eighteen months, its forces would have faced a Royal Navy stripped of any sea-based air power.[35] The aircraft carriers *Hermes* and *Invincible*, the backbone of the suc-

[33] *Ibid.*, 273. [34] Freedman (fn. 15), 199. [35] Record (fn. 24), 44.

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cessful British attempt to retake the islands, were slated for retirement and sale to Australia; the government had canceled plans and contracts for any new carrier construction. Furthermore, Argentina was in the process of acquiring new arms and would have been much better equipped a few months later. The latter fact and the concentration of the British fleet at home both point to a decision resting on a firm—and logical—conviction that the British would not fight.

A second assumption, ancillary to the first, was that the United States would prevent the British from going to war. As was the case with Israel's two linked assumptions in 1973, these two premises were plausible, interrelated, and powerful enough to drive out any second thoughts that may have materialized. When war broke out, *The Economist* depicted the U.S. as trying to play the role of "everybody's friend."[36]

Militarily, there was another key Argentine strategic error: the failure to move its air power from the mainland to the Falklands.[37] Because of this failure, the potential superiority of the Argentine air force was canceled out since it was forced to operate from the mainland, 400 miles from the Falklands. The war could have turned out quite differently if the air force had been moved to the islands, adding further support to the hypothesis that Argentina simply assumed that Britain did not intend to go to war.

Argentina also probably reasoned that its gunboat diplomacy would succeed. This was a lesson of recent history that was very well grounded and, in fact, made the invasion decision seem like a prospect with a very low cost, an extremely low probability of failure, and, given the intensity of Argentine feeling, a very high payoff. Throughout the world during the 1970s, five islands, groups of islands, or parts of islands had been seized successfully through an application of force that was "appropriate, limited, and naval."[38] Iran took several islands at the mouth of the Persian Gulf; China seized the Paracels; Vietnam occupied the Spratleys; Indonesia assumed control of East Timor; and Turkey took over a part of Cyprus. Protest greeted all of these operations and reversed none of

[36] "Falkland Islands: Will Two Weeks' Steaming Let Off the Pressure?" *The Economist*, April 10, 1983, 22; see also Franks (fn. 3), 75-76. Former President Leopoldo Galtieri, the leader of the military junta during the war, maintained that Argentina would not have invaded if the junta had known that the U.S. would support Britain ("Argentine Military Arrests Chief During Falklands War," *Washington Post*, April 13, 1983, A27). Prior to the invasion, the clear perception in Argentina was that the U.S. would not react. In fact, when the Argentine Ministry of Foreign Affairs unilaterally issued a communiqué on the New York talks, Iglesias Rouco in *La Prensa* discussed the advantages of a direct seizure of the Falklands, an act which he believed would be "understood" by the United States, which could be placated with an offer of joint naval facilities.

[37] Record (fn. 24), 44-45.

[38] Cable (fn. 30), 73.

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them. Precedents therefore abounded for the probability of success of such a venture.

For the British, then, warning signals existed but probably could not have been distinguished from previous invasion scares. For both sides, faulty assumptions were discernible (at least retrospectively). But we can ransack history's wars and ferret out fundamental but wrong assumptions as the preludes to almost every dyadic case that evolves into armed hostilities.

We know that the Falklands conflict occurred and that inevitably colors our reactions and interpretations. But if we look at the case from the vantage point of a British or even an Argentine analyst in January 1982, what would the analytical landscape have looked like? We would see the following general picture.

The Falkland Islands are a permanent crisis arena, one of about thirty bilateral conflicts that have arisen in Latin America since 1945. The majority of these are still active at some level; few have been resolved; some have tottered on the brink of war (such as Cuba and the U.S. in 1961, Guatemala and Belize from 1975 to 1977); a few have escalated into warfare (e.g., El Salvador and Honduras in 1969). Since 1832, in fact, there have been ten wars in the general area (five in the 1800s, three before 1945, and two since).[39]

In the period between 1965 and 1981, there has been more overt conflict, a fact which is unarguably related to the perceptible decline of U.S. hegemony in the region and the passing of the dominance of cold-war issues (compared to the 1948–1965 period). A new type of military regime has appeared in South America, one that actively promotes modernization. The former emphasis on internal security has been replaced by the achievement of an irreducible core of internal control and the advent of sharper interstate competition, heralding the transition from the doctrine to the diplomacy of national security.

Territorial claims and resource competition fuel many of these conflicts. Ideological and hegemonic conflict have been decreasing.[40] Conflicts oriented around migration can be expected to increase in number and intensity. Many conflict constellations are a mix of all five types. The Anglo-Argentine conflict is hegemonic, territorial, and resource-oriented in character.

We can portray the general contours of the Latin American conflict

[39] Grabendorff (fn. 14).

[40] Ideological conflicts include disputes between dictatorships and democracies, civilian and military regimes, and capitalist and socialist alternatives. Big power claims to supremacy or hegemonic conflicts include as special cases the hegemony of a colonial power (Britain) and the hegemony of a regional power (Brazil).

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environment in early 1982 fairly well. But this is a considerable distance from being able to estimate the probability that any specific conflict will escalate into a war. If we descend to the level of the Anglo-Argentine dyad, we soon discover that our ability to profile the relationship accurately does not provide any real basis for reliable, valid prediction.

A scholarly essay on the Anglo-Argentine conflict, which appeared in print several months prior to the war and covered the period from 1968 through 1981, characterized the dispute as one of "cooperative confrontation."[41] The overall assessment was that there were

> clear signs that both disputants are imbued with a willingness to settle in order to provide a foundation both for improved Anglo-Argentine relations and for the cooperative development of the islands' offshore economic potential. In practice, however, certain factors militate against a settlement. . . .[42]

At the same time, it was emphasized that

> there is still the matter of whether Argentina, which has proved more inflexible throughout the dispute, interprets Britain's search for a detente as a genuine gesture of conciliation rather than a sign of weakness to be exploited.[43]

It is highly unlikely that a current intelligence assessment in January 1982 would have been fundamentally different from the above academic account. Nor would the February negotiations have necessarily changed the outlook. British intelligence reports in the middle of March suggested that senior Argentine naval officers, consistently the most bellicose element of the military on the Falklands, "doubted that Argentina would invade the Falklands."[44] Whether attack warning indicators—immediate pre-crisis signals—could have penetrated the fog of wishful thinking on both sides is debatable. Clearly, trying to put oneself in the seat of the analyst with foresight responsibilities rather than hindsight knowledge invests "intelligence failures," the term that so many use so loosely, with a completely different aura.

Improvement must concentrate on doing whatever helps to make analysis better at the margin. One idea that is directly relevant to the Falklands case and also has much more general applicability concerns strategic assumptions and arguments. Stein, for instance, in assessing the quality of strategic analysis in Israel in 1973, focused on the logic of the prevailing strategic argument.[45]

The discussion of the 1973 war above, which is admittedly quite

[41] Beck (fn. 15).
[42] *Ibid.*, 40.
[43] *Ibid.*, 54.
[44] Franks (fn. 3), 47.
[45] Stein (fn. 5), 168.

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tentative and ad hoc in nature, does suggest that strategic assumptions played a significant role in the prewar analysis and decision processes. As Stein points out, flaws in the Israeli strategic logic were not attributable to the need to develop or flesh out the structure of the argument rapidly in a threat-saturated crisis context. The process of conceptualization was able to unfold at a leisurely pace.

With respect to the British and the Falklands, the same is true. In fact, compared to the Israelis, their political and military leaders had an appreciably longer period of time to develop, analyze, assess, and modify as necessary their strategic conceptual tools and the accompanying logic. The almost ritualistic quality of the assumption that Argentina would not resort to war and the lack of evidence that other assumptions and conceptual structures were considered worthy of sustained and careful (and comparative) analysis cannot be blamed on time pressures. However we might wish to characterize the origins, the conflict dates back a long time (to 1965, 1945, the 1880s, 1833, or the 1770s, depending on one's perspective). Stein's conclusion is clearly generalizable to this case: "Strategic arguments developed over time can be remedied over time and, if they can be, error may be at least partly avoidable."[46]

Although the explanatory power of strategic concepts seems to be impressive, Stein readily acknowledges that the relative impact of poor logic compared to other determinants of failures in estimation and analysis has yet to be ascertained for the prewar 1973 Israeli case. This caveat applies with even more force to this preliminary discussion of the Falklands case.

## Implications, Caveats, and Lessons

The Falklands war has made a solution of the dispute that led to it less likely than ever. Attitudes on both sides have hardened and the islanders, never supportive of any kind of a settlement that might introduce Argentine sovereignty, are even more negatively disposed toward Buenos Aires than before the conflict. The British had tried to substitute bluffing for a credible commitment and ended up with the very commitment they had consistently avoided. The Argentines attempted to seize what they saw as rightfully theirs and are now much further from the goal than before. If the war was launched to prop up General Galtieri's regime, as some have speculated, the outcome and its aftermath were about as inconsistent with that goal as could be imagined.

Warning intelligence functions to provide foresight and thereby pre-

[46] *Ibid.*

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vent war (if possible). It succeeded in accomplishing neither objective in this instance, although intelligence per se may not have been the primary or real culprit. While the analysis here has focused on attack warning, ignored wartime intelligence, and only touched on planning,[47] the Israeli-P.L.O. war of June 1982, the Falklands, and the ongoing Iran-Iraq conflict all reaffirm the importance of intelligence both prior to and during a conflict.[48]

Iraqi President Hussein's intelligence miscalculations included the capability of Iran's forces as well as his own and also the expected impact of his attack on Iran's domestic situation. For Israel, electronic intelligence was vital in determining how to destroy Syria's missiles in the Bekaa valley. In the Falklands, the British skillfully meshed analysis of photo reconnaissance pictures and reports from small units as the basis for their decision about the best possible landing site.

Defense planning, including the realistic assessment of one's own capabilities and the capabilities and intentions of the opposition, turned out to be crucial in these recent wars. Careful and honest net assessments and attention to strategy (both in the political and military senses of the concept) both proved absolutely indispensable. Neither Iraq nor, apparently, Argentina devoted enough thought to these issues.

The lessons and implications of the Falklands War have relevance beyond the direct participants. All of the evidence we have about international crises and wars since 1945 points to the conclusion that there was nothing that unique about this particular war. Images of the international crisis that draw on the Cuban missile crisis of 1962 or associate crises with the superpowers or the East-West conflict axis are not representative of the range or variety of crises in the contemporary international arena. Crises have occurred throughout the world and have involved confrontations between nations of all types. Especially within the third world, endemic conflicts have often erupted into crises and some have led to actual warfare.

Table 1 presents forty-eight crisis dyads from 1966 through 1978. These pairs of international crisis actors constituted the empirical database for a computerized crisis warning and monitoring system developed and tested by the U.S. Defense Advanced Research Projects Agency.[49] This list reflects a Western bias; crisis chronologies of Soviet,

[47] Betts (fn. 8, 63-65) makes this tripartite breakdown.
[48] Moodie (fn. 30), 31-32.
[49] Gerald W. Hopple, "Internal and External Crisis Early Warning and Monitoring," Technical Report (McLean, Va.: International Public Policy Research Corporation, 1980).

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Chinese, and other actors would differ appreciably.[50] Furthermore, the database is not designed to be exhaustive in coverage; only the most salient international crises during the time frame are featured.

Despite these qualifications, the list in Table 1 clearly depicts the volume and variety of crises in the global arena. The Middle East (defined

TABLE 1
CRISIS LISTS: 48 CRISIS DYADS, 1966–1978

| *Dyad* | *Starting Date* | *Comment* |
|---|---|---|
| Syria-Jordan | 12/1/66 | |
| P.L.O.-Jordan | | |
| U.S.S.R.-China | 1/11/67 | |
| Israel-Syria | 6/5/67 | |
| Israel-Egypt | | |
| Israel-Jordan | | |
| U.K.-China | 6/27/67 | Hong Kong dispute |
| Greece-Turkey | 11/15/67 | |
| Greece-Cyprus (Turkey) | | |
| Turkey-Cyprus | | |
| Israel-Jordan | 1/2/68 | |
| U.S.A.-North Korea | 1/23/68 | *Pueblo* |
| U.S.S.R.-Czechoslovakia | 8/20/68 | |
| Israel-Egypt | 2/24/69 | |
| Israel-Syria | | |
| U.S.S.R.-China | 3/2/69 | |
| Honduras-El Salvador | 6/30/69 | |
| North Vietnam-U.S.A. | 3/18/70 | |
| North Vietnam-Cambodia | | |
| North Vietnam-South Vietnam | | |
| Uganda-Tanzania | 7/9/70 | |
| Jordan-Syria | 9/1/70 | |
| Jordan-P.L.O. | | |
| Jordan-Iraq | | |
| U.S.A.-Syria | | |
| U.S.A.-U.S.S.R. | | |
| India-Pakistan | 11/21/71 | |
| Uganda-Tanzania | 9/17/72 | |
| Rhodesia-Zambia | 1/9/73 | |
| Israel-Egypt | 1/6/73 | |

[50] DARPA sponsored research on international crises as perceived by both the Chinese and the Soviets (in addition to crises as defined from the U.S. perspective); see Robert Mahoney, Jr., "Crisis Management: A Survey of Findings and Unresolved Problems," in Hopple and others (fn. 12).

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TABLE 1 (*cont.*)

| *Dyad* | *Starting Date* | *Comment* |
|---|---|---|
| Israel-Syria | | |
| U.S.A.-U.S.S.R. | 10/24/73 | Global alert |
| Greece-Turkey | 7/15/74 | |
| Cyprus-Cyprus (Turkey) | | |
| U.S.A.-Cambodia | 5/12/75 | *Mayaguez* |
| Morocco-Spain | 5/27/75 | |
| Morocco-Algeria | 12/9/75 | |
| Syria-Lebanon | 6/1/76 | |
| Syria-P.L.O. | | |
| Syria-Israel | | |
| Angola-Zaire | 3/10/77 | |
| Egypt-Libya | 7/19/77 | |
| Somalia-Ethiopia | 8/8/77 | |
| Israel-Syria | 9/19/77 | |
| Israel-P.L.O. | | |
| Uganda-Tanzania | 10/31/77 | |
| Cambodia-Vietnam | 12/28/78 | |

as crisis contexts involving Israel and one or more Arab opponents as well as related conflict episodes, such as the Syria-Jordan and P.L.O.-Jordan pairs in 1970) dominates the empirical universe of world crisis in the 1966 to 1978 period. The next most frequent type of crisis occurred between nonaligned third world opponents (a total of fourteen, including five Middle East cases). Included are the Honduras-El Salvador war (1969), several significant crises between Uganda and Tanzania, several additional crises involving African nations, and India and Pakistan.

In contrast, other crisis types are relatively rare. Only four cases of superpower crisis appear on the list (two between the United States and the Soviet Union and two between the Soviet Union and China). There are eight East-West crises and four involving internecine disputes within the communist world. All five Western crises are accounted for by dyads in the Greece-Turkey-Cyprus arena. Finally, two West-South crises (U.S.-Syria in 1973 and Morocco-Spain in 1975) occurred during the period. There were no East-South crises.

All of this reflects an international system in which power has become significantly more dispersed. The tight and loose bipolar systems of the 1945 to 1960 period enabled the superpowers to orchestrate international political trends to a much greater extent and forced a considerable amount of international crisis and conflict into the cold-war channel.

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As bipolarity eroded, conflicts within the West and East surfaced and intensified. The international system became multipolar in the 1960s and this polycentric pattern accelerated in the 1970s, increasing the probability and incidence of crises within the West, East, and South (as well as between the West and East). The diffusion of power process is continuing and the world is becoming increasingly complex; almost invariably, this pattern will dominate the 1980s. Our expectations about conflict and crisis must flow initially from this fundamental structural configuration.

The Anglo-Argentine crisis, then, was neither deviant nor unprecedented. Of the larger implications, two warrant mention. First, U.S. policy makers and analysts can expect to continue to confront secondary crises, including ones between friendly powers or allies. High priority will have to be given to analyzing and anticipating such possibilities. The goal should not be to achieve an impossible degree of perfect warning, but to enhance the ability to react rapidly and decisively. One way to do this is by more explicitly and systematically monitoring and analyzing potential and actual conflict and crisis arenas, thereby providing for more rational and effective contingency planning.

Wars between allies can be projected for the future. In fact, this is one of the predictable consequences of a transition from a bipolar to a polycentric system. Not surprisingly in the light of trends in the international system, the Sino-Soviet and French-American conflicts both became overt and then escalated in the 1960s. The Western European reaction to pressure from the U.S. on the Soviet pipeline deal, probes to the West for arms or economic aid and investment from Angola, Mozambique, and Iraq, daily pronouncements in Rumania and Israel, and a multitude of other messages and events, both subtle and blatant, constantly remind decision makers in the Kremlin and Washington that allies can be a decidedly mixed blessing. In addition, if people are close friends, their relationship has nowhere to go but down.

The logic of the latter statement provides the basis for Bruce Bueno de Mesquita's initial argument that war between allies should not be shocking.[51] He analyzes seventy-six outbreaks of war between states from 1816 to 1974, discovering that in twenty-three cases, each belligerent had at least one ally. Of these twenty-three cases, fifteeen involved a preexisting formal military agreement between the states at war. The fifteen instances of formal allies going to war represent 20 percent of the seventy-six wars. Even more revealing, allied dyads constituted only

[51] Bruce Bueno de Mesquita, *The War Trap* (New Haven and London: Yale University Press, 1981), 160.

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about 7 percent of all total annual country pairs in the international system from 1816 to 1974. Thus, wars between allies are about three times more likely than would be expected on the basis of the distribution of bilateral military agreements.

The second implication concerns the third world. A central arena for conflict, crisis, and war in the 1970s, the third world will undoubtedly be a more dangerous environment in the 1980s.[52] For many reasons, domestic stresses and strains on third-world societies will probably experience a significant growth. To mention just one cluster of potent causal factors, the debt structure and other international economic aspects of the domestic reality confronted by most countries of the South are currently deteriorating and are quite ominous in their implications for the future.

We can expect an upsurge of both internal and external crises and violence. Much of the growth of world military spending since 1968 can be attributed to developing states.[53] Every conceivable index—military expenditures (total and per capita), armed forces (total and per 1,000 of population), number of developing countries with advanced military systems (supersonic aircraft, missiles, armored fighting vehicles, modern warships), arms imports, and domestic defense production—dramatically highlights the increasing role and prominence of the third world in international security affairs. Given the runaway military trends and the expanding array and heightened lethality of weapons and armaments obtained by developing societies (and increasingly produced indigenously as well), conflict will probably not only be far more frequent—it will probably be much more destructive.[54]

No part of the third world is free from endemic internal and external conflict. This impacts directly on U.S. concerns both because the U.S. is a superpower with global interests and because the developing world has been and will continue to be the primary surrogate arena of competition between the U.S. and the Soviet Union.[55] The victory since 1975 of seven pro-Soviet communist parties in Asia and Africa only exac-

[52] Theodore H. Moran, "North-South Relations in the 1980s," *Naval War College Review* 35 (September-October 1982), 29-30.

[53] Edward A. Kolodziej and Robert Harkavy, "Developing States and the International Security System," in John J. Stremlau, ed., *The Foreign Policy Priorities of Third World States* (Boulder, Colo.: Westview, 1982), 19-47.

[54] Moodie (fn. 30) amends this proposition slightly by arguing that arms inventories of LDCs are likely to feature a combination of simple and sophisticated weaponry.

[55] For a particularly lucid and sophisticated analysis of recent Soviet activity in the third world, see Donald S. Zagoria, "Into the Breach: New Soviet Alliances in the Third World," in Erik P. Hoffman and Frederic J. Fleron, eds., *The Conduct of Soviet Foreign Policy* (New York: Aldine, 1980), 495-514.

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erbates the dangers and the potential for serious conflict and upheaval in strategically vital areas of the third world.

Was the Falklands war foreseeable and preventable? To what extent is any war predictable? The concept of analysis failure suggests that better analysis could have averted the outbreak of hostilities. Of course, we can never know this in a definitive sense. But the evidence of flawed analysis on both sides, the critical miscalculations made by both governments, and the wrong but reassuring assumptions that governed the two decision-making processes all point to such a failure.

Stein's juxtaposition of the intelligence and stupidity explanations for intelligence failure relates to this point.[56] The intelligence explanation emphasizes the intrinsically and inescapably complex, uncertain, and ambiguous nature of the task of intelligence and estimation. In contrast, interpretations that attribute failure to the "stupidity" of analysts or leaders stress endemic information-processing biases or the suboptimal performance of leaders in a particular situation. The second version of the stupidity argument relies on evidence of cognitive rigidity, excessive commitment to only one interpretation, or the inappropriate utilization of a single concept or indicator.

The emphasis here has been on suboptimal analytical performance. This is the one area where we have some room (if only a little) for improvement. We cannot wish or define ambiguity and uncertainty out of existence. Nor, apparently, can we fundamentally affect or reform cognitive psychological processes; the extensive experimental evidence from psychology demonstrates that people continue to rely on biased heuristics and other suboptimal information-processing routines even if they are informed and warned about them. But we can encourage better analysis when there is evidence of poor logic and other remediable shortcomings. People can be urged to think in terms of the multiple and competing hypotheses, especially in the area of strategic assumptions and concepts, that usually emerge in a long-term and relatively stress-free environment.

[56] Stein (fn. 5), 151.

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# SECULARIZATION AND STATES OF MODERNITY

By TERRANCE G. CARROLL*

THE only systematic comparative study of the relationship between religion and political development begins by asserting that "religion is in general an obstacle to modernization."[1] While Smith's work stands alone in scope and detail of analysis, this assumption is basic to most of the literature on political development and modernization.[2] Secularization is a prerequisite for the modern state, according to this literature, and the modern state is necessarily an essentially secular state.

There are a few studies that raise doubts about the validity of this generalization. Means found that Islam had mixed effects in Malaysia, in some ways encouraging modernization, and in some ways acting as an impediment. Nash came to a similar conclusion about the role of Buddhism in Burma, and Williams argued that Roman Catholicism is now a positive factor in nation-building in Latin America. More recently a number of authors have reacted to the widely discussed "Islamic revival" by arguing that Islam does not act as a general obstacle to modernization in the Middle East.[3]

This apparent contradiction between the findings of at least some studies of the role of specific religions in particular countries and the more general literature on political modernization raises the possibility

* I should like to acknowledge the thoughtful comments on an earlier version of this paper provided by two colleagues in the Politics Department at Brock University, Professors Juris Dreifelds and Roderick Church.

[1] Donald Eugene Smith, *Religion and Political Development* (Boston: Little, Brown, 1970), xi.

[2] For convenient and thoughtful surveys of the literature on political development, see Samuel P. Huntington and Jorge I. Dominguez, "Political Development," in Fred I. Greenstein and Nelson Polsby, eds., *Handbook of Political Science*, Vol. III (Reading, Mass.: Addison Wesley, 1975), 98-114, and Richard A. Higgott, *Political Development Theory* (London: Croom Helm, 1983).

[3] See Gordon P. Means, "The Role of Islam in the Political Development of Malaysia," *Comparative Politics* 1 (Summer 1969), 264-84; Manning Nash, "Buddhist Revitalization in the Nation State: The Burmese Experience," in Robert F. Spencer, ed., *Religion and Change in Contemporary Asia* (Minneapolis: University of Minnesota Press, 1971), 105-22; Edward J. Williams, "The Emergence of the Secular Nation-State and Latin American Catholicism," *Comparative Politics* 5 (January 1973), 261-77; and the collection of articles in John L. Esposito, ed., *Islam and Development: Religion and Sociopolitical Change* (Syracuse: Syracuse University Press, 1980).

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that the relationship between religion and political change may be considerably more complex than is usually suggested. In this article I argue that there is no *necessary* relationship between secularization and the development of a modern state. Rather, we must consider specific religions and their varying degrees of compatibility with particular characteristics of political modernity.

The analysis that follows rests on four main points. The first is that any conception of the modern state must inevitably reflect a particular cluster of ideological values, and each of the main Western ideologies posits its own version of political modernity. Secondly, I argue that the sole characteristic attributed to the modern state by *all* of the major Western ideologies is the capacity to undertake a wide range of large, complex tasks on a regular and continuing basis. Thirdly, by comparing specific third-world religions with this core characteristic of political modernity, we will discover that no serious, general incompatibility exists. Finally, an analysis of the relationship between particular ideological models of the modern state and specific religions will identify some dyads as having a moderate potential for conflict and others as largely compatible.

Eisenstadt has characterized the literature on political modernization as ahistorical, Western-centered, and poorly supported by the available evidence. He concluded that we require alternative models of the modern state.[4] While Eisenstadt's main concern was with the assumption that political modernity requires nation-building, his argument applies equally well to the political role of religion. If we are unwilling to limit our conception of the modern state to a particular ideological model, we must accept that the modern state is not necessarily secular. Depending upon the religion of the people and the ideological character of the state elite, a modern state may give primary importance to religion, it may be essentially secular, or it may be at any point between these extremes.

## The Central Concepts

One of the not insignificant difficulties in addressing this topic is that the key concepts have extremely complex and ambiguous meanings. Here I focus on the "modern state" rather than the broader concept of a "modern political system" or the more dynamic notions of "political

[4] S. N. Eisenstadt, "Varieties of Political Development," in S. N. Eisenstadt and Stein Rokkan, eds., *Building States and Nations*, Vol. I (Beverly Hills, Calif.: Sage, 1973), 41-48. See also Stein Rokkan, "Centre-Formation, Nation-Building and Cultural Diversity: Report on a UNESCO Programme," *ibid.*, 20, 26.

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modernization" and "political development."[5] In the pages that follow I identify several competing definitions of the modern state concept that are based in different ideologies, and I attempt to extract from these a widely accepted core meaning of the term. For the moment we can note that the development of a modern state is a virtually universal aspiration of third-world elites, and that these elites all seem to see such a state as one that has—among other things—the ability to do almost anything and everything that any existing state is capable of doing.

If the Swedish state can provide an efficient, humane, and universal welfare system, any modern state should be able to do likewise. If the Soviet, West German, and Japanese states can guide a process of extremely rapid economic expansion, within the limits imposed by their natural resources, every modern state should be able to achieve similar results. If the American state can supervise, stimulate, and assist an extraordinarily productive agricultural system, every modern state should be able to make as good use of available agricultural resources. It is not necessary that the governors of a modern state actually choose to do all of these things, but the limitations should be voluntary, rather than being imposed by political, administrative, or technical incapacities.

The second key concept is "secularization." Glasner has identified many of the different and sometimes contradictory meanings which have been assigned to this concept.[6] One of the greatest difficulties is distinguishing between secularization on the one hand, and religious reform or change on the other. For our purposes, secularization will be taken to mean any diminution in the scope of human life that is governed by religious laws, religious values, or religious authorities. Thus, if a change in religious laws had the effect of transforming a formerly religious matter into a question governed by secular authorities or individual judgment, it would be considered to be an instance of secularization, no matter how strong the theological justification for the change.

My central question is whether secularization is a prerequisite for the attainment of the distinctive characteristics of the modern state. When scholars argue that secularization is a necessary precondition for political modernization, they sometimes take as their baseline a historical or theoretical ideal type in which religion dominates all else. For example, Smith describes a "traditional religiopolitical system" as one in which:

[5] When, in the interests of euphony, I use the phrase "political modernity," this should be taken as a synonym for "the modern state."

[6] Peter E. Glasner, *The Sociology of Secularization: A Critique of a Concept* (London: Routledge & Kegan Paul, 1977).

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(1) ideology, including the legitimation of political authority, is derived entirely from religion; (2) the political and religious communities are inseparable, and religious conversion is akin to treason; (3) political stability results from the religiously integrated and legitimated social system, rather than from any political capability; (4) religious specialists fill many important political roles; and (5) the political ruler has extensive religious functions.[7]

The establishment of this sort of baseline may be very helpful in studies which trace the evolution of states to their current condition. Both history and logic would seem to indicate that some measure of secularization is required if a traditional religiopolitical system is to achieve even a minimal differentiation of political and religious institutions and roles. As long as political roles are largely filled by religious specialists, the skills and expertise of the political and administrative specialists are unlikely to develop. But virtually all existing states have already experienced secularization to at least this extent—or they have never known the complete integration of the religious and the political that we find in Smith's model.[8]

Those who posit a necessary relationship between secularization and political modernity certainly do not typically intend that this generalization should be taken to be of only historical interest. Thus, if we wish to evaluate the need for *additional* secularization in societies that have pre-modern states, our baseline should be the religiopolitical situation as it commonly exists in such countries today. For this reason, then, the starting point in this analysis will be the main religions of the third world—Buddhism, Catholicism, Hinduism, and Islam—in their contemporary manifestations. The question is whether some diminution in the existing political or social significance of these religions is necessary if countries of the third world are to develop modern states.

## States of Modernity

This section outlines four alternative models of the modern state. The first of these is a liberal conception of modernity so dominant in the literature on political modernization that it is sometimes taken to be a universal and nonideological description of the modern state. As Rudebeck has noted, "a highly positive evaluation of 'pluralist' liberal democracy is a common characteristic . . . of almost all established Western or U.S. political science concerned with political development."[9] By

[7] Smith (fn. 1), 6-7.

[8] Glasner (fn. 6), 67-76.

[9] Lars Rudebeck, "Political Development: Towards a Coherent and Relevant Theoretical Formulation of the Concept," *Scandinavian Political Studies* 5 (1970), 35.

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considering this understanding of the modern state within the context of a comparative review of competing models, we shall be able to distinguish the elements in each conception that are the subject of ideological dispute from those that are generally accepted. For ease of comparison we will focus on five key aspects of each ideological conception of political modernity: the nature of the political culture; the type of regime; the character of the political process; the structure of political institutions; and the scope of state activity.

## The Modern Liberal State

In the modern liberal state both elite and mass publics are expected to take an essentially rational approach to governance. Pre-modern man is pictured as ruled by custom, by primordial loyalties, and by religious values, while modern man is governed by rational self-interest, and by a secular and segmented system of values. Some authors value this liberal political culture in its own right, seeing it as an aspect of progress toward "secular humanism-pragmatism."[10] Others value it primarily as the underpinning for other features of a modern liberal state.

The regime is democratic, of course, with multiple opportunities for popular input and control. Within this framework we find a pluralist political process founded on the voluntary associations made possible by the individualism and pragmatism of the political culture. Political roles are differentiated from other, non-political aspects of life, and from each other. Differentiation of the political from the economic, religious, familial, and other spheres provides the large measure of subsystem autonomy required for a pluralist political process, while differentiation and specialization within political life is taken to be indispensable for the performance of large, complex tasks.[11] This reasoning also suggests that state institutions must be characterized by a rational bureaucratic structure.

Together these features make possible the extensive scope of state activity typical of modern liberalism. The state is expected to be able to plan, initiate, and control major economic and social changes (but not to plan in a comprehensive sense). It has the ability to extract resources from sophisticated and unwilling citizens; to provide social services in a dependable way throughout its domain; to regulate the economy and stimulate economic growth; and to protect the public from domestic and foreign threats to its security.

[10] The quoted phrase is from Smith (fn. 1), 85. For a similar view see Eisenstadt (fn. 4), 52.

[11] Alejandro Portes, "On the Sociology of National Development: Theories and Issues," *American Journal of Sociology* 82 (July 1976), 63-64.

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The empirical derivation of this model is apparent. It is essentially a somewhat idealized reflection of the contemporary American state. What is surprising is how few countries match this picture, even if we make allowances for its exaggerated or idealized character. None of the communist states can be considered modern by this account. The Soviet Union, Czechoslovakia, East Germany—not one of these is democratic; not one is pluralist; and not one is rational in the sense that the word has been used here. Nor do we find any countries with a non-European heritage which can be counted as modern. Japan displays few symptoms of the individualist/pluralist/rationalist syndrome that is central to a diagnosis of endemic modernity. Indeed, one must doubt if even the affluent Western European states, which have been strongly influenced by social democratic thought, can really be counted as modern by the terms of this model. To the extent that the main political issues revolve around the question of social class, pluralism is limited. To the extent that equality is taken to be the most important political value, rationally calculated, individual self-interest becomes a less important social force.

Other models of political modernity are required if we wish to include more than a very few countries in the category of those with a modern state. Let us see how proponents of the other main Western ideologies—Marxism, social democracy, and contemporary conservatism—conceive of the modern state.

## The Modern Marxist State

As is true of most ideologies, Marxism has given rise to a great range of alterations and reformulations. For the mainstream Marxist-Leninist influenced by the Soviet model, however, political modernity takes the form of a socialist state. Full realization of a communist society remains a goal for the future. The political culture of the modern Marxist state is characterized by a general commitment to the value of equality; a loyalty to the Communist Party as the main force resisting capitalism, imperialism, and revisionist backsliding; and a dedication to the collective pursuit of the communist utopia. Marxist culture may be "rational," but it is rationality of a different order from the individual self-interest of the liberal model.[12]

Nor is the Marxist state "democratic" in the liberal sense. The regime

[12] For examples of how Soviet Marxist scholars conceive of a modern political culture, see E. Chekharin, *The Soviet Political System Under Developed Socialism* (Moscow: Progress Publishers, 1977), 219-78, and Maxim Kim, *The Soviet People—A New Historical Community*, trans. D. Fidlon (Moscow: Progress Publishers, 1974), 191-203. On the meaning of "rationality" in the Soviet system, see Paul Cocks, "Bureaucracy and Party Control," in Carmelo Mesa-Lago and Carl Beck, eds., *Comparative Socialist Systems: Essays on Politics and Economics* (Pittsburgh: University of Pittsburgh Center for International Studies, 1975), 215-48.

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is characterized by democratic centralism and the authoritative guidance of the vanguard who make up the Communist Party. Disagreement and debate take place within the Party, but once a decision is reached all members must unite behind it. Since opposition from outside the Party is considered illegitimate, this is clearly not a pluralist process. Competing systems of values, be they political or religious, are illegitimate.[13] The state claims the right to regulate all aspects of life, and this totalitarianism implies that the differentiation of political institutions from other social structures is more limited than in the liberal model. Within the state structure we do find highly differentiated and specialized administrative agencies, however, and the modern Marxist state has a political capacity even more extensive than that of the liberal state.

### The Modern Social Democratic State

The contrast between the liberal and Marxist models is sharp; they differ on almost every dimension of political modernity. The social democratic conception of the modern state coincides with liberalism in some respects, is similar to Marxism in others, and is unique in still others. The political culture of a social democracy values equality above all else—except the procedures of democracy itself.[14] It is characterized by a sharp consciousness of the shared interests of the working class, and a collectivist conception of the structure of society. The democratic structure of the regime is similar to the liberal model, but the political process takes the form of a competition between classes rather than a pluralist pursuit of individual interests.

As a nontotalitarian state, social democracy exhibits a degree of institutional differentiation that is closer to liberalism than to Marxism, but lacks the ideological commitment to subsystem autonomy characteristic of pluralist liberalism. The political and economic sectors are extensively integrated, and there is no objection in principle to state activity in other social sectors. As is true of both the liberal and the Marxist models, the modern social democratic state has the capacity to undertake large, complex tasks with a frequency and continuity inconceivable in a pre-modern state. It is unique, however, in that this state

[13] This theme is explored in M. I. Basmanov and B. M. Leibzon, *The Revolutionary Vanguard: Battle of Ideologies*, trans. D. Skvirsky (Moscow: Progress Publishers, 1977), and in G. Shahnazarov, *Socialist Democracy: Aspects of Theory*, trans. B. Bean (Moscow: Progress Publishers, 1974), 44-54.

[14] Richard Lowenthal, "The Postwar Transformation of European Social Democracy," in Bogdan Denitch, ed., *Democratic Socialism: The Mass Left in Advanced Industrial Societies* (Montclair: Allenheld, Osmun, 1981), 33. The classic statement of the principles of Swedish social democrats is Herbert Tingsten, *The Swedish Social Democrats: The Ideological Development*, trans. G. Frankel and P. Howard-Rosen (Totowa, N.J.: Bedminster Press, 1973), esp. 345-461.

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capacity is ideologically committed to the creation and maintenance of a comprehensive social welfare system as its primary responsibility.[15]

## The Modern Conservative State

The Marxist and social democratic models of political modernity can be abstracted with relative ease because we have obvious cases of countries that approximate these models (the Soviet Union and Sweden, for example), and because we have extensive and explicit statements of the ideological foundations of these states. The nature of conservatism is such that we lack clear and accepted formulations of its principles, on the other hand, and the nature of the modern Western world makes it difficult to find countries that are both modern and conservative. Nevertheless, many conservative philosophers do agree on a set of basic principles that are distinct from those of other Western ideologies,[16] and some countries do seem to embody these principles to a greater extent than most Western societies. Ireland and Switzerland might serve as examples, even though they are not so ideologically pure as our specimens of liberal, Marxist, and social democratic states.

The primary values of a modern conservative political culture are stability and order. Change may be justified on pragmatic grounds, but not on the basis of abstract principles, rational planning, or individual whim.[17] Society is conceived in collective or communal terms, and political identification with ascriptive groups based on ethnicity, religion, language, race, and nationality is important and legitimate.[18] Inequality is likely to be taken to be an inherent characteristic of mankind, but in a modern conservative state this view is almost invariably accompanied by a belief that natural inequalities should not be permitted to produce extreme hardship for the less able. Democracy is valued primarily as a procedure for indirect control and legitimation, and the regime is likely to emphasize representative institutions rather than direct popular input or control (the Swiss example notwithstanding).[19]

The nature of the political process depends heavily on the character

[15] See Gunnar Myrdal, "What is Wrong with the Welfare State?" in Nathanael Greene, ed., *European Socialism Since World War I* (Chicago: Quadrangle Books, 1971), 198-207.

[16] This *distinctly* conservative body of thought is to be found largely in the works of British and European writers. In the United States the terms "conservative" and "liberal" are commonly applied to political positions that differ from each other in ways that are interesting and important, but that are both essentially liberal in their fundamental principles.

[17] For discussions of British and European conservatives' perspectives on stability and change, see Michael Oakeshott, *Rationalism in Politics and Other Essays* (London: Methuen, 1962), 182-96, and R.E.M. Irving, *The Christian Democratic Parties of Western Europe* (London: George Allen & Unwin, 1979), 33.

[18] Irving, 51-56.

[19] Michael P. Fogarty, *Christian Democracy in Western Europe, 1820–1953* (Notre Dame, Ind.: University of Notre Dame Press, 1957), 89-96.

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of the main opposition to the conservative majority, but the conservative forces are likely to emphasize the competence of their leaders and their all-embracing concern for the welfare of the society. Disputes within the conservative segment of society will typically be handled by way of discussion, compromise, and cooperation within the elite rather than by means of public debate or mass action. Leaders of nonpolitical sectors may well be included in this process of elite interaction, thus limiting the extent to which political life is differentiated. The scope of state activity is more restricted in the conservative model than in any other ideological version of the modern state, but this is a matter of choice rather than incapacity.[20] Like its ideological competitors, the modern conservative state has the capacity to undertake large, complex tasks when these seem necessary.

## The Core Character of Political Modernity

Each of the main Western ideologies posits a significantly different conception of the nature of the modern state. The political culture ranges from the rational and individual self-interest of liberalism to the collective egalitarianism of Marxism. Regimes vary from authoritarian to democratic. The political process may be typified by class conflict, by pluralism, or by firm one-party dominance. There is no reason to suppose that these differences exhaust the possible range of variation in states that might plausibly be termed "modern." These are the articulated ideologies associated with states for which such a claim might now be made, but it is possible—indeed, it is likely—that at least some of the ideologies now emerging in the third world will eventually also prove compatible with political modernity. The main conclusion suggested by this comparison is simply that we should anticipate that political development will take a variety of forms, rather than converging toward the dominant liberal model.[21]

Nevertheless, the existing range of variation is not infinite. The four ideological models are in considerable agreement on one characteristic of the modern state. It is this commonality that allows us to assign at least a limited meaning to the concept. All four ideologies emphasize the extensive capacity of the modern state. The range of variation is from the self-imposed limits of the conservative model to the all-encompassing efforts of a Marxist state. As important as this difference is for Western countries, it is a relatively small distinction from a wider

[20] Oakeshott (fn. 17), 184.

[21] Crawford Young, *Ideology and Development* (New Haven: Yale University Press, 1982), provides a stimulating comparison of the effects of different ideological frameworks on the performance of African states.

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perspective. Even the modern conservative state has a capacity unknown in less developed countries. The following characteristics distinguish the capacity of a modern state from that of a pre-modern state. The former can deal simultaneously with a variety of complex tasks; it can regularize and routinize the performance of these tasks; and it can carry out these tasks on a continuing and long-term basis. In these respects its capacity is unlike that of the Egyptian pharaonic states which constructed the pyramids, or the Pol Pot regime which temporarily transformed an entire society.

One can argue, then, that the core character of the differing concepts of political modernity is the existence of a very extensive capacity to undertake large, complex tasks on a regular and continuing basis.[22] Each of the main Western ideologies would posit this feature as a part of a model of the modern state, and each would add to it values which are specific to the ideology in question. If we focus on this core characteristic, which is valued by all four ideologies and by the publics and leaders of most countries throughout the world, we make it possible to employ a positively valued conception of the modern state without necessarily accepting any particular ideology in toto. In the context of this article, we make it possible to distinguish a contradiction between religion and this core element from a contradiction between religion and the values of a particular ideology. Is secularization a prerequisite for the genus "political modernity," or is it required for only some specific ideological species of modern state?

## Religion and the Modern State

What things are implied when we say that a state has an extensive capacity? It seems to me that, at a minimum, such a state must have well-developed capabilities in the areas of communication, coercion, legitimation, planning, and policy implementation.[23] If a state is to have

[22] Concepts of development and modernization that emphasize the capacity of the state have been criticized on the grounds that they ignore the input side of politics, and imply a preference for control rather than human choice. On the latter point see Brian Loveman, "The Comparative Administration Group, Development Administration, and Antidevelopment," *Public Administration Review* 36 (November/December 1976), 616-21. My analysis suggests that the nature of input processes cannot be specified without entering into an ideological debate. An emphasis on state capacity is not value free, as Loveman's argument shows, but neither is it a focus of dispute between widely held ideologies.

[23] This list of "essential capabilities" seems not to be identical to those suggested by any other author, probably because of my desire to exclude ideologically contested elements, and focus on state capacity. My thinking on this matter was influenced by a great many authors, among the most helpful of whom were David Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965), and Ferrel Heady, *Public Administration: A Comparative Perspective*, 2d ed. (New York: Marcel Dekker, 1979).

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the capacity typical of political modernity it will have to be capable of communicating regularly with most of its citizens, and of receiving input and feedback from most important societal groups. Compliance with state policies and, ultimately, the very survival of the state, require a mixture of coercive and legitimative capabilities. "Planning" is used in a very broad sense here, incorporating the identification of problems that require a political response; the identification of the characteristics of a satisfactory response to each such problem; the development of policies and programs that have a reasonable potential for producing a satisfactory response; and an assessment of the resources required to implement such policies. In other words, a modern state must be capable of identifying at least medium-term goals, and of developing moderately effective policies for achieving these goals. Finally, of course, a modern state must be capable of implementing these policies in such a way that they are normally at least partly successful in producing the intended outcomes.

To what extent and in what ways is secularization a prerequisite for the attainment of these capabilities which, together, give a state the capacity to regularly undertake a variety of large, complex, continuing tasks? Table 1 displays (in an admittedly rough way) the relationships between the main third-world religions and these five capabilities.

TABLE 1
THE EFFECT OF RELIGIONS ON POLITICAL CAPABILITIES

| | *Buddhism* | *Catholicism* | *Hinduism* | *Islam* |
|---|---|---|---|---|
| Communication | 0 | + + | 0 | + |
| Coercion | 0 | = | 0 | + |
| Legitimation | + | + + | + | + + |
| Planning | 0 | 0 | 0 | = = |
| Implementation | 0 | 0 | = | 0 |
| Overall Effect | + | + + + | 0 | + + |

Note: + indicates a positive effect, 0 a neutral effect, and = a negative effect.

## COMMUNICATION

These religions may have a significant effect on the communications capability of the state in two ways, both of which are positive. A religion may provide an efficient network for the transmission of messages from the authorities to the public if most members of the society gather

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regularly for religious services, and if religious leaders take a view of their sphere of competence sufficiently broad to encompass a wide range of messages. Islam and Catholicism meet these criteria, and thus may provide a state with a greater capability for communication than it would otherwise have. Buddhism and Hinduism are neutral in this regard. Their emphasis on private forms of religious observance, combined with irregular or intermittent public gatherings, force the state to find or create some other communications network. In some Buddhist societies (such as Sri Lanka) there are reasonably regular and well-attended congregations, but the scope of Theravada Buddhist preaching (*bana*) is too narrow and too prescribed to offer much potential for political communication.[24]

The second way that religion may improve communication is by providing reasonably reliable channels for the transmission of information from the society to the political authorities. This requires a hierarchically structured clergy so that the authorities can receive messages from the society as a whole by consulting with a limited number of religious authorities. By filtering information the religious hierarchy may also distort it, but in the absence of such a hierarchy the state can receive messages as easily from such other sources as village headmen or tribal chiefs. Third-world societies do not lack local spokesmen, but they often lack structures for aggregating public opinion and demands. It is this need that a religious hierarchy can partially meet. Because they lack clerical hierarchies, Mahayana Buddhism, Hinduism, and Sunni Islam have little effect on the ability of the state to receive communications from the society. Theravada Buddhism sometimes has such a hierarchy, but only in countries like Thailand and pre-Khmer Rouge Cambodia where the state has taken it upon itself to impose a structure on the monks.[25] Only Catholicism and Shi'ite Islam have the potential to make a significant contribution to this side of the communicative capacity of the state.

### Coercion

Buddhism and Hinduism have little effect on the coercive capabilities of the state. Both promulgate religious norms against extreme brutality but, in the absence of an institutional means of expressing opposition to excessively harsh measures taken by the state, the efficacy of these norms

[24] Anthony Fernando, "Contemporary Buddhism in Sri Lanka (Ceylon)," in Heinrich Dumoulin, ed., *Buddhism in the Modern World* (New York: Macmillan, 1976), 66-72.

[25] In the collection edited by Dumoulin, *ibid.*, see Donald K. Swearer, "Recent Developments in Thai Buddhism," 99-108, and Marcello Zago, "Contemporary Khmer Buddhism," 109-19.

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depends largely on the extent to which they have been internalized by members of the elite. In an Islamic society, on the other hand, all law is held to be fundamentally religious—the state simply implements and enforces the *sharia*—and Islam explicitly justifies the use of force to uphold religious law. By contrast, the Catholic Church now has particularly strong strictures against extreme brutality by the state, and it has an institutional structure capable of providing leadership in opposition to such measures.[26] In these ways, then, Islam may make a mildly positive contribution to the coercive capability of the state, and Catholicism may place an equally mild inhibition on some types of coercion by the authorities.

### Legitimation

Like most religions, those prevalent in the third world help to create a general ethos of legitimacy for the authority of the state, provided that the state does not function in a manner clearly incompatible with important principles of the religion of its citizens. All four of these religions suggest to their followers that shortcomings in material well-being are of less importance than might otherwise be thought, and they all teach that it is right and proper to obey secular authorities as long as this does not require irreligious behavior. They are all open to manipulation by the authorities who, by couching their policies in religious terms and inducing the endorsement of religious leaders, may be able to create an impression of religious sanction for actions of the state.[27] Islam and Catholicism go farther in this respect because both have the ability to actually give religious sanction to state policies. Catholicism and Shi'ite Islam have this potential as a consequence of the existence of clerical hierarchies. Their religious authorities have the right to speak for the religion as a whole, and function within a tradition which accepts that it is proper for them to give explicit approval to a ruler or policy that is in accordance with religious doctrine. Sunni Islam lacks this hierarchy, but its doctrine explicitly holds that it is the duty of every Muslim to obey any worldly leader who does not break Islamic laws.

It is equally true, on the other hand, that all four religions can also act as delegitimizing forces. The existence of widely accepted religious norms provides a potential for condemning a regime as not just unwise

[26] On Islam and state coercion, see Hamoodur Rahman, "The Islamic Concept of State," *Hamdard Islamicus* 2 (Spring 1979), 55. The contemporary Roman Catholic position is outlined in Brian Smith, "Churches and Human Rights in Latin America: Recent Trends on the Subcontinent," in Donald H. Levine, ed., *Churches and Politics in Latin America* (Beverly Hills: Sage, 1980), 155-93.

[27] Milton J. Yinger, *The Scientific Study of Religion* (New York: Macmillan, 1970), 409ff.

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or incompetent, but as immoral.[28] Buddhism presents the smallest threat to the legitimacy of the state since it lacks both codified law and a clerical hierarchy; the religious law of Hinduism and Sunni Islam can provide a rationale for opponents of the state who wish to challenge its legitimacy; and Catholicism and Shi'ite Islam present the greatest threat since they possess both of the relevant characteristics. On average, however, authorities are in a better position to capture and employ a religion to legitimate their actions than are dissidents. Thus, Catholicism and Islam may make a moderate positive contribution to the legitimizing capability of the state, while Buddhism and Hinduism probably have a weaker—but still positive—legitimizing effect.

### Planning

Religion may affect the planning capability of the state in two main ways. The first is through education and the socialization of cultural modes of thinking. Planning requires rational and scientific patterns of thought. By emphasizing contemplative, mystical, magical, or ritualistic ways of viewing the world, religion may create cultural obstacles to the development of a large pool of trained personnel who are accustomed to dealing with problems through a positivist, ends-means process. Nevertheless, this obstacle is so slight, even in the third world, that it can be ignored if it is not reinforced by other aspects of religion. India has had essentially secular education for more than a century and Hinduism no longer has any significant impact on the society's acceptance of scientific education. Buddhist schools resisted scientific and secular education for longer in some countries, but more on traditional and nationalistic grounds than because of religious doctrine. Buddhism too has largely adapted.[29]

Islam still offers some resistance to scientific education, despite the reasonably successful process of educational reform experienced in many Islamic societies. In Islam every aspect of life is intimately related to and governed by religion. This creates an inescapable tension between the desire to study economics, or medicine, or chemistry *per se*, and the desire to study specifically Islamic tenets of economics, medicine, or science. Moreover, until the last few decades the mosque provided much of what education was available in Islamic societies, and the curriculum was made up largely of the study of Islamic law and traditions. To the

[28] Michael C. Hudson makes this point with regard to Islam in his "Islam and Political Development," in Esposito (fn. 3), 14-20.

[29] See Smith (fn. 1), 104-7, and Peter A. Pardue, *Buddhism: A Historical Introduction to Buddhist Values and the Social and Political Forms They Have Assumed in Asia* (New York: Macmillan, 1968), 137.

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extent that scientific education competes with the traditional curriculum for the time available to students, there is a clash between the two even if there is no direct, substantive conflict. Edens reports, for example, that the proportion of a Saudi primary school student's time devoted to religious instruction declined from over three-quarters in 1948 to about one-quarter in the 1960s.[30]

Catholicism, on the other hand, has the potential to make a mildly positive contribution to scientific education, and thus to the planning capability of the state. Because it is Western-centered, unlike the other three, Catholicism adapted earlier and more completely to the social need for scientific training. As a result, Catholic societies often have extensive Church-sponsored education systems that emphasize science as well as religion.

The second main way in which religion may affect the planning capability of the state is by constraining policy choices or challenging the authority of the state in particular policy fields. All law-bound religions do this to a limited extent simply by identifying some potential policies as irreligious or immoral. Buddhism lacks such a religious code, of course, and even though Hinduism has a code of religious law, only a very few significant policy options face religious resistance of any importance under it. Catholicism has a greater potential in this area because it combines formal religious strictures against a number of policies commonly adopted by developing states (such as family planning, secular education, and civil divorce laws) with an organized clerical hierarchy capable of presenting a formidable opposition to the state. Both Sunni and Shi'ite Islam have even more extensive codes of law that attempt to place restrictions on state activity in a great many aspects of life, and the latter is also endowed with organized religious leadership.

In these ways, then, Islam and Catholicism may challenge or restrict the autonomy of the state. At the same time, the seriousness of this challenge should not be exaggerated. As the Saudi experience has demonstrated, a careful choice of tactics by the state elite can sometimes bring about the acquiescence of religious leaders to policies they have initially condemned.[31] In sum, then, Islam may mildly hinder the planning capability of the state by resisting scientific education and restricting policy alternatives. Catholicism is likely to encourage scientific education while also restricting policy choices, thus ending up in about the same neutral position as Buddhism and Hinduism.

[30] D. G. Edens, "The Anatomy of the Saudi Revolution," *International Journal of Middle East Studies* 5 (January 1974), 50-64.

[31] Ronald R. MacIntyre, "Saudi Arabia," in Mohammed Ayoob, ed., *The Politics of Islamic Reassertion* (London: Croom Helm, 1981), 17.

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### Implementation

The main implication of this final capability is that a modern state must have an efficient, effective, and honest bureaucracy. All four religions attempt to guide their believers away from corruption, but without notable success. Buddhism has no other apparent connection with the implementation capability. Again we find, however, that by virtue of their extensive codes of religious law, the other three religions may affect this capability—and again we find that the likely effects are mixed. Catholicism, Hinduism, and Islam may all help to inculcate a general disposition to follow formal rules, an important characteristic of any bureaucracy. At the same time, however, one of the great dangers in bureaucratic organizations is that a formalistic, ritualistic devotion to rules may displace concern for the substantive outcome of the procedures. These formalistic, ritualistic religions probably contribute to this problem by encouraging faithful obedience to unchanging rules.[32] Thus their positive and negative influences on the implementation capability would seem to be roughly in balance.

One specific aspect of Hinduism remains to be noted; to a limited extent it continues to legitimate caste divisions and a caste hierarchy. While many Hindu businessmen are able to separate their secular professional lives from their private religious convictions, incongruities between caste and bureaucratic hierarchies still create some difficulties.[33] For this reason Hinduism may have a mildly negative effect on the capability to implement policies, while the other main third-world religions seem to have little or no overall effect.

### Religion and State Capacity

There may be a danger that the very act of identifying these various tendencies will create an impression that religion is a powerful determinant of political capabilities. In fact, the most striking conclusion to be drawn from this analysis is that *no religion has any strong and consistent effect on the capacity of the state*. The relationships, whether positive or negative, never seem to be of more than moderate strength. A second important finding is the complexity of relationships among third-world religions and state capabilities. No one religion has the same effect on

[32] Stanley J. Heginbotham, *Cultures in Conflict: The Four Faces of Indian Bureaucracy* (New York: Columbia University Press, 1975), 20-34.

[33] Milton Singer discusses the compartmentalization of religious and professional life in his *When a Great Tradition Modernizes: An Anthropological Approach to Indian Civilization* (New York: Praeger, 1972), 315-50. For an examination of the difficulties created by caste see G. K. Prasad, *Bureaucracy in India: A Sociological Study* (New Delhi: Sterling Publishers, 1974), 112-15.

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each of the five capabilities; no capability is affected in the same way by all four religions. The features that most frequently underlie an apparent relationship between religion and the capabilities of a modern state are the existence of a formal code of religious law and a clerical hierarchy, but each of these factors has both positive and negative effects. Broad generalizations about the relationship between religion and political modernity seem unlikely to survive careful empirical testing.

While the analysis of relationships has been unavoidably crude, and the estimation of their strength is extremely impressionistic and tentative, this attempt to "unpack" the religion/modernity association suggests that the presence of one of these religions is neither a necessary nor a sufficient condition for shortcomings in state capabilities. Indeed, on balance Catholicism and Islam may have a mildly positive influence on the capacity of the state, while Buddhism and Hinduism are likely to have little or no overall effect. Certainly this analysis provides no support for the argument that further secularization is necessary if third-world states are to become politically modern.

## Secularization and Ideology

Why, then, is there such widespread agreement that secularization is an essential prerequisite to political modernization? Part of the answer, I suspect, is simply that ideal types are too often confused with reality.[34] Religion does not play the role assigned to it in the ideal-type traditional system in any existing society; nor is any existing society as completely secularized as the ideal-type modern state. A second factor is that particular characteristics of some religions may well have a negative impact on state capacities. A more detailed assessment suggests that these are likely to be roughly balanced by other, positive relationships. The main factor, however, seems to be that models of political modernity reflect Western ideologies, with liberalism most prominent. The status of religion in many societies probably is incompatible with the development of some specific ideological versions of the modern state.

This is undoubtedly clearest in the case of the Marxist model. The modern Marxist state claims the right to govern all aspects of life, including the religious, and it is inherently opposed to the theistic, transcendent, and mystical aspects of religion.[35] This is entirely unacceptable for the Catholic or Muslim, of course, and it also creates conflicts

[34] Glasner (fn. 6), 67-89.

[35] J. M. Bochenski, "Marxism-Leninism and Religion," in Bohdan R. Bociurkiw and John Strong, eds., *Religion and Atheism in the U.S.S.R. and Eastern Europe* (Toronto: University of Toronto Press, 1975), 1-17.

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with Buddhism and Hinduism. Buddhism and Hinduism claim no knowledge of absolute truth, and both are non-theistic, or polytheistic in a tolerant way. This has led some Buddhists and Hindus, as well as some Marxists, to argue that these religions are compatible with Marxism.[36] In fact, however, in their popular versions these religions are usually theistic and mystical, and these characteristics are ultimately unacceptable to the Marxist. The fundamental basis for the conflict is the totalitarianism of the Marxist state. Some Buddhists and some Hindus may have few objections to Marxism, but the Marxist state will inevitably attempt to weaken, and eventually to eliminate, the role of these religions.[37] In the case of Hinduism the conflict is exacerbated by the fact that ascriptive caste identities remain politically important, even though the nature of caste is undergoing a fundamental transformation.

Turning to the liberal model of the modern state, we can first note its general compatibility with Buddhism. The religious individualism of Buddhism coincides with the political individualism of the liberal state. Religious rejection of absolute truth reinforces political pragmatism. Equality in the search for personal salvation bolsters equality of opportunity in politics and economics.

Hinduism shares some of the liberal attributes of Buddhism, but is less congruent with the liberal model in other respects. The ascriptive nature of caste is certainly illiberal, but the political role of caste seems to be becoming akin to that of voluntary associations in the liberal model.[38] The existence of a code of religious law also conflicts with the liberal reliance on legitimation by majority rule. Since independence, however, Indian governments have displaced aspects of the religious code with secular law with such frequency that compliance with Hindu law is now widely perceived to be an essentially voluntary matter. In much the same fashion the Hindu emphasis on obligations to the community has been weakened by the absence of any systematic enforcement

[36] On Buddhism and Marxism see Fred R. von der Mehden, *Religion and Nationalism in Southeast Asia: Burma, Indonesia, the Philippines* (Madison: University of Wisconsin Press, 1963), 54-73. Sankar Ghose discusses Hinduism and Marxism in his *Socialism and Communism in India* (Calcutta: Allied Publishers, 1971), 27ff.

[37] See, for example, Holmes Welch, *Buddhism Under Mao* (Cambridge, Mass.: Harvard University Press, 1972), 364-80; and, in the Bociurkiw and Strong collection (fn. 35), Alexandre Bennigsen, "Islam in the Soviet Union: The Religious Factor and the Nationality Problem," 91-100, and Gerhard Simon, "The Catholic Church and the Communist State in the Soviet Union and Eastern Europe," 190-221.

[38] This argument is associated with Lloyd I. Rudolph and Susanne Hoeber Rudolph, *The Modernity of Tradition: Political Development in India* (Chicago: University of Chicago Press, 1967), 103-31. Relevant empirical evidence may be found in Anil Bhatt, *Caste, Class and Politics: An Empirical Profile of Social Stratification in Modern India* (Delhi: Manohar Book Service, 1975), 127-99.

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mechanism. In sum, then, contemporary Hinduism retains some illiberal features, but these seem too weak to constitute a significant obstacle to the development of a modern liberal state.[39]

Catholicism and Islam are much less congruent with the modern liberal state. A liberal political culture gives primary importance to individualism, and accepts the free pursuit of individual self-interest as the highest value. Catholicism and Islam both view society in collective terms; they both suggest that the good of the community is more than just an aggregation of individual wants, and that at least some interests of the community properly take precedence over personal freedom and self-interest. In the modern liberal state legitimacy is derived from the competitive processes of pluralist democracy. As law-bound religions promulgating a revealed and unchanging truth, Catholicism and Islam both claim a higher source of legitimacy. No matter what process is employed to create it, secular law is illegitimate if it conflicts with sacred law.[40] Because they view society in collective terms, and because they propound a transcendent code of law, these two religions also tend to oppose the separation of church and state typical of pluralist liberalism. Some degree of institutional differentiation is permissible, but the relegation of religious norms to voluntaristic aspects of life is not. The scope and normative power of Islamic law, and the institutional strengths of Catholicism and Shi'ite Islam, are such that the lack of congruity with liberalism may constitute an obstacle of some importance.

Catholicism and Islam may hinder the development of a modern liberal state, then, while all four third-world religions seem incompatible with a Marxist conception of political modernity. In the case of the conservative and social democratic models of the modern state, on the other hand, there appear to be few incongruities with any of these religions. The historical experience of each religion is largely with conservative regimes, and each has developed traditions and doctrines that complement those of conservatism. The fact that the conservative takes the maintenance of religious norms to be an important political value naturally strengthens this alliance.

Buddhism and Islam also serve as particularly good complements to social democracy. Buddhism takes no position on property rights, while an important stream in Islamic thought holds that the good of the community takes precedence over individual rights of ownership. Both

[39] This may be an example of secularization being required and taking place. In some other Hindu societies (such as Nepal) religion may still constitute a significant obstacle to liberalism.

[40] John Hicks, "Notions of Legitimacy in Islamic and Liberal-Democratic Political Thought," *Journal of South Asian and Middle Eastern Studies* 3 (Summer 1980), 9-22.

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religions contain important egalitarian principles that reinforce the social democratic emphasis on equality.[41] Historically Catholicism and Hinduism were less compatible with social democracy, but modern reforms have greatly reduced the sources of conflict. The battle that used to rage between the authoritarian, hierarchical, and sacral Roman Catholic Church, and democratic, egalitarian, materialistic socialism, has been largely resolved. Indeed, modern Catholic concern with poverty, oppression, and other social ills of the third world often leads the Church to endorse social democratic economic and social policies. What opposition remains tends to take the form of an anachronistic hostility to the term "socialism." In the case of Hinduism, caste was the main historical obstacle to social democracy, but the ongoing transformation of caste has gone far to eliminate the conflict. It is true that Hinduism—and indeed, the other third-world religions—would deny that social class should have the central role accorded it in social democratic theory and practice, but to the extent that the lower classes constitute the bulk of a society, it is possible to argue that class and community interests coincide.

## Conclusion

We have seen, then, that each of the main Western ideologies posits its own, unique picture of the nature of the modern state. Religion sometimes presents an obstacle to specific ideological components of political modernity. While the precise relationship depends upon the religion in question, it is generally true that the Marxist and, to a lesser extent, the liberal models of the modern state may require a significant degree of secularization. Modern conservative and social democratic states seem to be largely compatible with the main religions of the third world.

The various ideological conceptions of political modernity all emphasize one core characteristic—a capacity to undertake a range of large, complex tasks on a regular and continuing basis. This capacity seems to rest on five distinct capabilities, and different religions affect each of these capabilities in different ways. In general, however, the relationship between religion and the capacity of the state seems to be neutral or mildly positive. Secularization does not seem to be a general prerequisite for the development of the capacity typical of the modern state.

Religion is but one of many factors that affect the prospects for change in the capacity of a state, and this analysis suggests that it is of less

[41] See Smith (fn. 1), 195-200, 224-26, and John Thomas Cummings, Hassein Askari and Ahmad Mustafa, "Islam and Modern Economic Change," in Esposito (fn. 3), 34-44.

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importance than is commonly thought. In cases in which the dominant religion is incompatible with the ideological character of the state elite, expansion of the capacity of the state may be more difficult, and it may require extensive secularization. In cases in which the dominant religion is compatible with the dominant ideology, there is one less obstacle to the development of a modern state. Countries in this category may develop extensive state capacities without experiencing significant secularization. And if the attempt to expand the capacity of the state should fail, we must look to factors other than religion as the cause.

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# INTERNATIONAL RELATIONS THEORY, FOREIGN POLICY SUBSTITUTABILITY, AND "NICE" LAWS

By BENJAMIN A. MOST and HARVEY STARR*

## Introduction

A pair of basic problems appear to have impeded the development of an integrative understanding of international and foreign policy phenomena. The first has to do with the potential for foreign policy substitutability: through time and across space, similar factors could plausibly be expected to trigger different foreign policy acts. The second problem concerns the potential existence of "sometimes true," domain-specific laws. It is the logical opposite of the substitution problem: different processes could plausibly be expected to lead to similar results. Neither problem appears to be well understood in the current literature; if anything, standard research practices suggest that both are ignored entirely. Nevertheless, they are potentially important. Taken together, they suggest that scholars who are interested in developing a cumulative base of integrative knowledge about foreign policy and international relations phenomena should recognize that:

1. If writers on the foreign policy behaviors of governments define "islands of theory" in terms of concrete phenomena, their work will not yield broad understanding. If governments behave differently to pursue their (perhaps heterogeneous) national goals and, *under at least certain conditions* nations may *substitute* one means for another, then all of the behaviors that tend to be studied in fragmented fashion need to be conceived from the outset—*not* as separate and distinct phenomena, the understanding of which will eventually be integrated—but rather as commensurable behaviors or component parts of abstract conceptual puzzles.
2. Applications of the standard approaches for testing models and hypotheses are likely to produce misleading results and lead analysts to reject theories and models that are "good," "nice," and "useful," even

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if they are not general, or universally "true." If it is plausible to argue that states may pursue different goals, for different reasons and with different degrees of effectiveness, then it may be useful to reconsider the efforts to search for a "true," "general," or universally applicable explanation of what they do. It might instead be more sensible to search for models or theories that operate, hold, or are valid only under certain explicitly prescribed conditions.

As a consequence of our analysis, we conclude that there is a need to reexamine some of the "grand" theoretical approaches found in the "traditional" literature. A new synthesis of tradition and science and of grand, middle, and narrow approaches seems to be needed. Finally, in contrast to arguments such as those presented by Waltz and Singer, we find that the most fruitful avenues for theorizing and research are at the microlevel, in which the focus is on decision making, expected utility calculations, and foreign policy interaction processes.[1]

## Common Research Practices

Several basic research practices and procedures have increasingly come to characterize much of the current quantitative research on foreign policy and international phenomena.[2]

*Empirical "puzzles" and empirically defined islands.* Recent theoretical and quantitative empirical research on foreign policy and international relations has tended to focus on the "middle level" in an apparent effort to develop "islands of theory," or solutions to middle-range "puzzles." While scholars argue that they are ultimately concerned with understanding why states do what they do, they have eschewed efforts at "grand theory." They have tended to specialize their research—becoming arms race experts, alliance theorists, war analysts, students of arms transfers, specialists on the uses of foreign economic policy, experts on processes of international negotiation and conflict resolution, and so on.[3]

[1] Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979); J. David Singer, "Accounting for International War: The State of the Discipline," *Journal of Peace Research* 18 (No. 1, 1981), 1-18.

[2] Exceptions could readily be cited. Our contention is only that the practices outlined below are typical of much of what is done, and that they seem to reflect how scholars approach their research problems.

[3] Analysts who work with the WEIS, CREON, and COPDAB events data sets adopt a different approach, of course. Rosenau's "pre-theory" of foreign policy and its various extensions and tests also quite clearly depart from the pattern by hypothesizing that different explanations may hold in different types of states. See James N. Rosenau, "Pre-Theories and Theories of Foreign Policy," in Rosenau, ed., *The Scientific Study of Foreign Policy* (New York: Free Press, 1971), 95-149; James N. Rosenau and Gary Hoggard, "Foreign Policy Behavior in Dyadic Relationships: Testing a Pre-Theoretical Extension," in Rosenau, ed., *Comparing Foreign Policies* (New York: John Wiley & Sons, 1974), 117-49; Maurice A. East,

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Some have asked why nations go to war. Others have dealt with why alliances form or dissolve. Possible explanations and effects of arms transfers have been analyzed by a third group of researchers. Still others have focused on arms races, and so on.[4]

Some scholars specialize and focus on one phenomenon or type of event because the subject seems to them to have an intrinsic appeal. They regard the phenomenon, in and of itself, as a concept; their typical goals are to understand its causes or consequences. Other analysts, viewing their chosen empirical behavior or event as an indicator of some more overarching concept, reason that an understanding of the phenomenon will eventually inform them about the concept; examples are scholars who focus on war from the perspective of Galtung's structural theory of aggression.[5] They argue that progress toward an understanding of conceptual relationships will come most rapidly if they focus narrowly, concentrate on a given empirical phenomenon, and study it in isolation. At a minimum, an island of theory will emerge; an area of understanding will be established. If, in the meantime, other analysts have been successful in investigating other concrete behaviors and have produced islands of their own, it should eventually be possible to go further: the

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"Size and Foreign Policy Behavior," *World Politics* 25 (July 1973), 556-76. Our contention is only that such researchers tend to be the exception rather than the rule. It should also be said that a number of analysts report nongeneral findings; e.g., results that hold in the 19th century but not in the 20th, or that apply to major powers but not to minor ones. The point to note, however, is that the majority of these analysts initially look for—and apparently expect to find—general relationships. They have no *initial* theoretical expectation that their model *should* be applicable to some limited domain or that it should be useful only under certain conditions.

[4] For research that adopts a narrow focus on war, see Benjamin A. Most and Harvey Starr, "Diffusion, Reinforcement, Geopolitics, and the Spread of War," *American Political Science Review* 74 (December 1980), 932-46, and Harvey Starr and Benjamin A. Most, "Contagion and Border Effects on Contemporary African Conflict," *Comparative Political Studies* 16 (April 1983), 92-117. For a study dealing exclusively with alliances, see Brian L. Job, "Membership in Inter-Nation Alliances, 1815-1965: An Exploration Utilizing Mathematical Models," in Dina A. Zinnes and John V. Gillespie, eds., *Mathematical Models in International Relations* (New York: Praeger, 1976), 74-109. For a study focusing exclusively on arms races, see Philip A. Schrodt, "Richardson's Model as a Markov Process," *ibid.*, 156-75. Research dealing exclusively with international arms transfers is reported by Ilan Peleg, "Military Production in Third World Countries," in Pat McGowan and Charles W. Kegley, eds., *Threats, Weapons and Foreign Policy* (Beverly Hills, Calif.: Sage Publications, 1980), 209-30. For an analysis focused exclusively on international negotiations, see Glenn H. Snyder and Paul Diesing, *Conflict Among Nations* (Princeton: Princeton University Press, 1977). Analysts who have written about "islands of theory" in international relations include Harold Guetzkow, "Long Range Research in International Relations," *American Perspective* 4 (Fall 1950), 421-40, and Nigel Forward, *The Field of Nations* (Boston: Little, Brown, 1971).

[5] See, for example, Maurice A. East, "Status Discrepancy and Violence in the International System: An Empirical Analysis," in James N. Rosenau, Vincent Davis, and Maurice A. East, eds., *The Analysis of International Politics* (New York: Free Press, 1972), 299-319, and Michael D. Wallace, *War and Rank Among Nations* (Lexington, Mass.: Lexington Books, 1973).

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"war" island would be linked with other middle-range theories of alliance behavior, arms transfers, defense expenditures, and so on. This theoretical synthesis, or bridging of the islands of middle-range theory, is expected to provide a more abstract, broad-gauge, or overarching understanding of foreign policy and international behavior.

*Empirical generalizations, laws and "always-true" theories.* The tendency to define puzzles or research questions in terms of particular, concrete empirical phenomena and to focus on one—but only one—type of event at a time combines with the fact that most scholars who study foreign policy and international relations systematically or scientifically seem to think of social laws and theories as being either true or false.[6] As we have noted previously, the search for *empirical* generalizations seems to have become the *sine qua non* of at least those scholars who study international relations quantitatively. To understand or explain a phenomenon such as an overt military attack, they consider it necessary to identify the factor(s) that, alone or in various combinations, correlate with the occurrences of attacks or that seem generally useful—according to some statistical criterion—for postdicting the occurrences of attacks in the data set in question. Although a few exceptions may be noted, the most common view is that a "good" explanandum is one that is generally associated through time or across space with attacks; a "less than good" explanandum is one that is not. Research proceeds by identifying and retaining the former while rejecting and abandoning the latter.[7]

On first consideration, these practices seem largely commonsensical. They do not seem to be at all problematic. Indeed, the practices are so uncontroversial that they are seldom discussed in the current literature. Researchers may be led astray, however, if they narrowly focus their research on only one type of empirical foreign policy behavior, define

[6] See James S. Coleman, *Introduction to Mathematical Sociology* (Glencoe, Ill.: The Free Press of Glencoe, 1964), 516.

[7] We are referring here to standard uses of the so-called "general covering law" or "refutationist" approach developed by scholars such as Hempel and Popper. For a brief rehearsal, see: Gregory A. Raymond, "Introduction: Comparative Analysis and Nomological Explanation," in Charles W. Kegley, Jr., Gregory A. Raymond, Robert M. Rood, and Richard A. Skinner, eds., *International Events and the Comparative Analysis of Foreign Policy* (Columbia: University of South Carolina Press, 1975), 41-51. The reasoning underlying this approach can be crudely reconstructed in syllogistic form: If a "true" (i.e., universal) law exists between some concepts *X* and *Y*, then that law should *always* hold in the empirical world and should be evidenced by whatever are the appropriate associations between the respective operational indicators of the concepts, *xi* and *yi*. Empirical analyses are conducted to test for such associations between the occurrences of the *xi* and *yi* in the expectation that those results will inform us about whether or not a law actually exists between *X* and *Y*. The absence of the association between the *xi* and *yi* allows us, if the reasoning is sound, to reject the *X*/*Y* law, while the appearance of the anticipated *xi*/*yi* relationship allows us to say that the data are consistent with, or support the existence of, the *X*/*Y* law.

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problems or potential islands in terms of such concrete phenomena, search exclusively for empirical-level generalizations, and equate laws with universal truths.

## Problem No. 1:
### THE OVERLOOKED POTENTIAL FOR POLICY SUBSTITUTION

The practices described above are likely to lead to a more abstract understanding of why states do what they do *only if* they do not substitute one empirically distinct foreign policy or behavior for another in pursuit of their (perhaps heterogeneous) national goals. If foreign policies can indeed be alternative routes by which decision makers attain their goals, then it would seem plausible that decision makers who are confronted with some problem or subjected to some stimulus could, *under at least certain conditions, substitute one such means for another*. If that is the case, any factor, stimulus, or problem that triggers *some particular* type of empirical foreign policy response could, under different conditions, trigger other, *apparently* distinct, *apparently* incommensurable behaviors. If the argument is valid, *similar* factors could lead to *distinct* concrete or empirical foreign policy responses.

The confounding effects of this potential for foreign policy substitutability can hardly be overemphasized. For example, Realists have argued that states seek the goal of security in the Westphalian self-help system. To the extent that states do indeed pursue such a grand or meta-conceptual goal ($Y$), and *in principle* have a variety of partially substitutable, alternative means—war ($y1$), alliance formation ($y2$), arms importation ($y3$), arms increases ($y4$)—for attaining the goal, a given presumed causal factor ($x1$; e.g., an increase in an opponent's arms) could logically be expected to explain why the decision makers of a given state would be willing to adopt *some* attempt to resolve the problem, $yi$. Because the stimulus ($x1$) could be expected to trigger the adoption of $y1$ by some decision makers while others might choose different options such as $y2, y3, \ldots, yn$, however, an understanding of *which particular* $yi$ would be adopted by different decision makers under different conditions would involve a consideration of how they make comparisons across, and eventually choose from, the range of those available options. It would require that analysts focus on more than a single type or form of foreign policy behavior, rethink the strategy of focusing on particular empirical phenomena, and reconsider the construction of middle-range islands of theory on the basis of concrete, unabstracted phenomena.[8]

[8] If $y1, y2, \ldots, yn$ are all potential means for attaining an assumed foreign policy goal, then each $yi$ is a potentially valid indicator of $Y$ (e.g., the initiation of an overt military

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Two examples should help to clarify these points. First, let us imagine a simple, totally deterministic, world in which the development of a "problem" ($X$) is invariably sufficient—but perhaps not empirically necessary—for an effort to resolve the problem ($Y$). Wherever and whenever $X$ occurs, $Y$ follows. The only complication is that efforts to resolve the problem ($Y$) can take any of three different empirical forms ($y1$, $y2$, $y3$) which are not necessarily mutually exclusive (i.e., an actor could respond to $X$ by adopting any one or any combination of them). If (1) $x1$ is a valid and reliable indicator of $X$, (2) $y1$, $y2$, and $y3$ are valid and reliable complementary indicators of $Y$, and (3) $x1$ and $-x1$ denote the presence or absence of the problem $X$ while $yi$ and $-yi$ denote the presence or absence of a given attempt to resolve the problem, the following case structures are possible:

| | | | | | |
|---|---|---|---|---|---|
| Case 1 | $x1$ | $y1$ | $-y2$ | $-y3$ | $Y$ |
| Case 2 | $x1$ | $-y1$ | $y2$ | $-y3$ | $Y$ |
| Case 3 | $x1$ | $-y1$ | $-y2$ | $y3$ | $Y$ |
| Case 4 | $x1$ | $y1$ | $y2$ | $y3$ | $Y$ |
| Case 5 | $-x1$ | $-y1$ | $-y2$ | $-y3$ | $-Y$ |
| Case 6 | $-x1$ | $y1$ | $-y2$ | $-y3$ | $Y$ |
| Case 7 | $-x1$ | $-y1$ | $y2$ | $-y3$ | $Y$ |
| Case 8 | $-x1$ | $-y1$ | $-y2$ | $y3$ | $Y$ |

Analysts who recognize the complementarity of the $yi$ and focus on abstract attempts to resolve the problem ($Y$)—rather than on the specific empirical forms that such solutions take—would not be at all confused by this pattern; each time $x1$ appears, $Y$ follows. Because $Y$ also appears when $x1$ is not present, however, these analysts would conclude that $X$, as indicated by $x1$, is sufficient (but not necessary) for $Y$. Other analysts who focus narrowly and specialize in attempts to understand the occurrence of any one—and only one—of the concrete $yi$ behaviors would run the risk of overlooking the relationship, however; the occurrence of $x1$ sometimes—but not always—precedes any particular $yi$. In short, $X$, as indicated by $x1$, would *not* appear to be sufficient (or necessary) for any one of the $yi$.[9]

strike or an increase in defense spending are complementary indicators of the concept "efforts to maintain or enhance security" or "foreign policy means"). Once this is recognized, the problem is easily understood: A focus on any one $yi$ fails to provide full coverage of the range of possible behaviors in the "means" concept. Analysts who focus on only one $yi$ are thus focusing only on "successful" occurrences. To that extent, an examination of the possible sufficiency relationship between any presumed causal factor ($xi$) and the $yi$ is precluded. The point here is analogous to our criticism of analysts who focus only on actual occurrences of war. See Benjamin A. Most and Harvey Starr, "Case Selection, Conceptualization and Basic Logic in the Study of War," *American Journal of Political Science* 26 (November 1982), 834-56, and "Conceptualizing War: Consequences for Theory and Research," *Journal of Conflict Resolution* 27 (March 1983), 137-59.

[9] The reader may think that researchers could readily resolve this problem by constructing

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A consideration of the hypothetical interactions among states *m*, *i*, and *j* in columns 1 to 3 in part A of Table 1 may clarify the problem further. The *m*th state increases its defense spending in each alternative round. States *i* and *j* respond by increasing their own expenditures (at $t_2$), by offering to form an alliance (at $t_4$), by dissolving an alliance or seeking foreign arms acquisitions respectively (at $t_6$) and, respectively, by seeking to negotiate or attack (at $t_8$).

Recognizing that the illustration is purely hypothetical and does not exhaust the full range of states' possible foreign policy behaviors, the basic point should be obvious: the situation would appear chaotic to analysts who ignored or overlooked the substitutability potential and focused on only a single type of behavior. The typical arms race theorist who focuses on reciprocated increases in defense spending would discover that increases by one state sometimes do, but sometimes do not, lead to similar responses by other states. The alliance theorist would discover that an increase in defense spending antedates an effort to form an alliance in one instance, but that other such increases precede the breakup of an alliance or states' complete inactivity in the alliance sphere. The war theorist and the expert on arms transfers would encounter similar complexities. Stepped-up defense spending precedes an attack in one case but also antedates conciliatory efforts in another; spending increases generally do not, but in one case does, appear to lead to an effort to import arms.

Thus, analysts who elect to focus on only one of the behaviors shown in the table are likely to be led astray. The strategy would make sense only if decision makers of all states responded identically to identical stimuli and were unable to substitute. If, however, the behaviors of decision makers of different states could in fact take a variety of forms, there would be no reason to expect a systematic, sufficient relationship between any initial condition and *any other* specific form of foreign policy behavior. To the extent that decision makers have some latitude in their choice of options and are sometimes able to substitute one alternative for another, a given factor could be expected to lead to, stimulate, or "cause" a variety of empirically distinct foreign policy acts, events, or behaviors.

The substitutability phenomenon, then, provides a possible answer to

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some composite scale of the various *yi*. In the past, the fashion would have been to factor analyze the *yi*. Such a strategy would work only if it is reasonable to assume that a given phenomenon *yi* invariably indicates the concept Y. As the discussion in the next section suggests, such a postulate might not be plausible. While the initiation of conflict may sometimes reflect the desire of decision makers to establish (or reestablish) their state's external security or viability, for example, the same behavior could reflect their desire to increase their government's viability in the face of domestic pressures.

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TABLE 1
HYPOTHETICAL ACTION/REACTION PROCESS
A

| | *States* | | | | |
|---|---|---|---|---|---|
| *Time* | *m* | *i* | *j* | *p* | *q* |
| *t*1 | *x*4 | | | | |
| *t*2 | | *x*4 | *x*4 | 00 | 00 |
| *t*3 | *x*4 | | | | |
| *t*4 | | *x*5 | *x*5 | 00 | 00 |
| *t*5 | *x*4 | | | | |
| *t*6 | | *x*6 | *x*7 | 00 | 00 |
| *t*7 | *x*4 | | | | |
| *t*8 | | *x*8 | *x*9 | 00 | 00 |

*x*4: Increase defense spending
*x*5: Offer to form an alliance
*x*6: Dissolve an alliance with state *k*
*x*7: Seek to import arms
*x*8: Seek to negotiate outstanding grievances
*x*9: Initiate an attack on the threatening state
00: No behavior

B

| | *States* | | | | |
|---|---|---|---|---|---|
| *Time* | *m* | *i* | *j* | *p* | *q* |
| *t*1 | *X*2 | | | | |
| *t*2 | | *X*2 | *X*2 | 00 | 00 |
| *t*3 | *X*2 | | | | |
| *t*4 | | *X*2 | *X*2 | 00 | 00 |
| *t*5 | *X*2 | | | | |
| *t*6 | | *X*2 | *X*2 | 00 | 00 |
| *t*7 | *X*2 | | | | |
| *t*8 | | *X*3 | *X*3 | 00 | 00 |

*X*2: Increase national defense capacity (*C*)
*X*3: Decrease national defense risk (*R*)
00: No behavior

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C

| | States | | | | |
|---|---|---|---|---|---|
| *Time* | *m* | *i* | *j* | *p* | *q* |
| *t*1 | *X*1 | | | | |
| *t*2 | | *X*1 | *X*1 | *X*1 | *X*1 |
| *t*3 | *X*1 | | | | |
| *t*4 | | *X*1 | *X*1 | *X*1 | *X*1 |
| *t*5 | *X*1 | | | | |
| *t*6 | | *X*1 | *X*1 | *X*1 | *X*1 |
| *t*7 | *X*1 | | | | |
| *t*8 | | *X*1 | *X*1 | *X*1 | *X*1 |

*X*1: Decision makers attempt to have $C > R$

D

PERIOD-END STATUS

| | States | | | | |
|---|---|---|---|---|---|
| *Time* | *m* | *i* | *j* | *p* | *q* |
| *t*1 | $C>R$ | $C<R$ | $C<R$ | $C<R$ | $C>R$ |
| *t*2 | $C<R$ | $C>R$ | $C>R$ | $C<R$ | $C>R$ |
| *t*3 | $C>R$ | $C<R$ | $C<R$ | $C<R$ | $C>R$ |
| *t*4 | $C<R$ | $C>R$ | $C>R$ | $C<R$ | $C>R$ |
| *t*5 | $C>R$ | $C<R$ | $C<R$ | $C<R$ | $C>R$ |
| *t*6 | $C<R$ | $C>R$ | $C>R$ | $C<R$ | $C>R$ |
| *t*7 | $C>R$ | $C<R$ | $C<R$ | $C<R$ | $C>R$ |
| *t*8 | $C<R$ | $C>R$ | $C>R$ | $C<R$ | $C>R$ |

the most intriguing of Zinnes's "puzzles." Posing the question, "Do nations interact?" she examines the work on arms races—an area in which substitutability could have important effects—and concludes that "the overall and overwhelming result is that nations *do not interact* in this domain."[10] The few studies that do discover interaction do not focus exclusively on armaments but on *overall* interaction patterns which are based on a wide range of hostile/cooperative events.

Although a thorough understanding of the best apparent solution to this difficulty must await the consideration of a few additional points,

[10] Dina A. Zinnes, "Three Puzzles in Search of a Researcher," *International Studies Quarterly* 24 (September 1980), 315-42, at 323 (emphasis in original).

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its basic outlines can be traced. *If* scholars are genuinely interested in understanding why states do what they do, they need to move beyond efforts to focus separately on particular concrete behaviors. Rather than asking middle-range questions about specific empirical phenomena, they should begin with the initial "grand" question with which they were allegedly concerned in the first place; rather than asking why states arm, form alliances, import arms, negotiate, attack, and so on, they should begin by asking what each behavior does, or at least could, represent.

Thus, we need to return to the more generic conceptualizations, such as "response to threat" or "response to uncertainty under the Westphalian security dilemma," that appeared in the traditional literature under the rubric of "security," "national interest," or "balance of power." A whole range of activities that are currently discussed under headings such as trade, aid, international law, regimes, international organization, and so on, could be conceptualized more broadly as "adaptation for coordination or collaboration" or some similarly overarching concept. The point to be emphasized, however, is the need to reconceptualize *exactly what* it is that we want to study, and *why*. Students of international relations have, in many cases, actually reified the operational indicators of international interaction. We have studied war *qua* war, alliances *qua* alliances, and have tended to overlook the broader international processes and phenomena that such specific forms of behavior represent.

The results of this narrowness have been recognized in recent reviews of the alliance literature by Ward and Job.[11] Both authors note that there has been little cumulation or theoretical development. Both conclude that alliance research is focused too narrowly—and rather unfruitfully—on questions such as the alliance/war relationship. While neither calls explicitly for reconceptual efforts, both suggest that the time may have come to move beyond the study of alliances *per se* and efforts to develop alliance theory. Job in particular seems to recommend asking what alliances do (or under certain conceptualizations, could) "really" represent.[12]

[11] Michael Don Ward, "Research Gaps in Alliance Dynamics: Literature Review," mimeo. (Evanston, Ill.: Northwestern University, 1980), and Brian L. Job, "Grins Without Cats: In Pursuit of Knowledge of International Alliances," in P. Terrence Hopmann, Dina A. Zinnes, and J. David Singer, eds., *Cumulation in International Relations Research*, Monograph Series in World Affairs, Vol. 18, Book 3 (Graduate School of International Studies, University of Denver, 1981).

[12] The need to reverse the tendency to attack international and foreign policy phenomena in fragmented, piecemeal fashion can be defended on other grounds. The point has been tellingly made by both Ostrom and Boynton. See Elinor Ostrom, "Beyond Positivism: An Introduction to This Volume," in Ostrom, ed., *Strategies of Political Inquiry* (Beverly Hills, Calif.: Sage Publications, 1982), 11-28, and G. Robert Boynton, "Linking Problem Definition and Research Activities: Using Formal Languages," in Judith A. Gillespie and Dina A.

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The beginnings of one such unifying formulation are shown in Table 2. The "image" is one of value-maximizing decision makers who perceive themselves to be confronted with a security dilemma. A perceived imbalance in the Capacity-to-Risk inequality (i.e., $C \leqslant R$) acts as the motive force; given an imbalance, decision makers are inclined or "willing" to attempt to reverse it by adopting some policy initiative that is likely to increase their defense capacity or decrease their risks. This "probable" behavior is expected to interact with what is "possible."[13] Put simply, decision makers will be constrained by the range of available policy options; they will adopt a given policy alternative if, and only if, they have both the "willingness" and the "opportunity" to do so.[14]

Assuming for the moment that the axioms in Table 2 apply to states $m$, $i$, and $j$, that each state has unlimited implementation capability or opportunities to act, and that $m$ views $i$ and $j$ as risks while $i$ and $j$ view $m$—but not each other—as possible sources of danger, the interactions among states $m$, $i$, and $j$ in part A of Table 1 begin to become intelligible. The apparently diverse behaviors and acts become commensurable. If the formulation and the just-mentioned auxiliary conditions had been

Zinnes, eds., *Missing Elements in Political Inquiry* (Beverly Hills, Calif.: Sage Publications, 1982), 43-60. Perhaps J. Bronowski, however, puts the point most clearly:

> All science is the search for unity in hidden likenesses. . . . The scientist looks for order in appearances of nature by exploring such likenesses. For order does not display itself; if it can be said to be there at all, it is not there for the mere looking. There is no way of pointing a finger or a camera at it; order must be discovered and, in a deep sense, it must be created. What we see, as we see it, is mere disorder. . . . [Full grown] science grows from a comparison. It has seized a likeness between two unlike appearances. . . . The progress of science is the discovery at each step of a new order which gives unity to what seemed unlike (Bronowski, *Science and Human Values* [New York: Harper Torchbooks, 1965], 13-15).

[13] Harold and Margaret Sprout, *An Ecological Paradigm for the Study of International Politics*, Center of International Studies, Princeton University, Research Monograph No. 30 (Princeton, 1968), and Harold and Margaret Sprout, "Environmental Factors in the Study of International Politics," in James N. Roseneau, ed., *International Politics and Foreign Policy*, rev. ed. (New York: The Free Press, 1969), 41-56.

[14] The careful reader will note that major elements of "Realism" and the "security dilemma" are being formalized in Table 2. See, for example, Hans J. Morgenthau, *Politics Among Nations*, 5th ed. (New York: Alfred A. Knopf, 1973); Robert Jervis, "Cooperation Under the Security Dilemma," *World Politics* 30 (January 1978), 167-214, and Robert O. Keohane, "Theory of World Politics: Structural Realism and Beyond" (Paper presented at the Annual Meetings of the American Political Science Association, Denver, 1982). These elements are being formalized around an expected utility framework that draws on the "cognitive behaviorism" of the Sprouts (fn. 13) in a manner eschewed by Realist theorists. While the model is somewhat similar to the Realist approach, therefore, it is also consistent with the Sprouts' dictum regarding environmental determinism: i.e., motivating factors need to be mapped through decision makers' perceptions *even if* all decision makers perceive alike and they do not make a (statistical) difference. Mapping through the decision makers is a *logical* requirement, not a statistical concern; nothing would happen (no relationships would hold) if decision makers did not exist. (We depart from Sprout and Sprout, however, insofar as we see policy initiations as "outcomes" of the interplay between what decision makers want to do and are capable of doing.)

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TABLE 2

A UNIFIED ACTOR/NATIONAL SECURITY DILEMMA FORMULATION

Axiom 1: The decision makers of an *n*th state are, or can be treated as, unified and value-maximizing actors who possess perfect information regarding all options and their consequences.

Axiom 2: At any given point in time, the decision makers in an *n*th state perceive that they have a certain national defense capacity (*Cnt*) and are confronted by some degree of national risk or vulnerability (*Rnt*).[a]

Axiom 3: The decision makers of an *n*th state are motivated or willing to establish the following inequality:

$$Cnt > Rnt$$

i.e., the decision makers want to be unconditionally viable and maintain at least local supremacy.

Postulate 1: Only if the decision makers of an *n*th state command the necessary objective capability or opportunity (0) to adopt some unilateral policy initiative, then the adoption of that initiative.

Postulate 2: If the decision makers of an *n*th state perceive that their state's defense capacity has been neglected or allowed to deteriorate between $t0$ and $t1$, they will perceive that $Cnt0 > Cnt1$.

Postulate 3: If a given state increases its national defense capacity between $t0$ and $t1$ and the decision makers in an *n*th state perceive that action as a threat, they will perceive that $Rnt0 < Rnt1$.

Postulate 4: If $Cnt1 < Rnt1$, then the decision makers of the *n*th state will be motivated to adopt some policy initiative designed to increase *Cn* and/or decrease *Rn* at $t2$; i.e., if the decision makers perceive that they are only conditionally viable at $t1$, then they will be willing to increase their capacities or decrease their risks.

Postulate 5: If, and only if, the decision makers of an *n*th state command the necessary capacity (or opportunity) to adopt some unilateral policy initiative and they are motivated (or willing) to do so, then the adoption of that initiative.

[a] Given the purpose of the illustration, it is not important to consider how one might operationalize either *Cnt* or *Rnt*. Only three points need to be noted. First, it should be understood that both capacities (*C*) and risks (*R*) are conceptualized here in terms of decision makers' perceptions. Put simply, a state's defense capacities and the risks it confronts are as they are recognized and understood by the decision makers. Consistent with this conceptualization, the capacities of "friendly" or distant states may be discounted by an *n*th state's decision makers in their calculation of *Rnt*; similarly, the capacities of friendly, and especially allied, states could be counted by the *n*th state's decision makers as supplemental sources of *Cnt*. Finally, it should be clear that capacities (*C*) and risks (*R*) may differ from the decision makers' objective capabilities or opportunities (see Postulate 1), which affect policy initiatives regardless of whether or not they are (accurately) perceived by the decision makers.

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in place at the outset, it would have been possible to deduce that each state would adopt *some* measure to increase capacity ($C$) or decrease risk ($R$) in each successive round (see part B of Table 1). Each of these concepts would have brought together a number of hitherto apparently diverse behaviors. Within the context of the model, increases in defense expenditures, offers of alliance, and efforts to import arms could have been seen as commensurable; they could have been recognized as alternative indicators of decision makers' efforts to increase their defense capacity. Alliance dissolutions, offers to negotiate, and military strikes could also have been rendered commensurable as efforts to decrease risk. It would have been possible to go further; efforts to increase capacity or decrease risk—along with the distinct empirical behaviors that each embraces within the formulation—could have been linked in turn to decision makers' efforts to manage their respective $C$-to-$R$ inequalities (see part C of Table 1).

With all of this in mind, consider the behaviors of nations $p$ and $q$ in columns 4 and 5 of Table 1 above. While states $m$, $i$, and $j$ display a wide range of behaviors—presumably in an effort to manipulate their Capacity-to-Risk ratios—$p$ and $q$ remain completely inactive. Viewed at the raw empirical level, there appears to be no consistency whatsoever in the actions of the five states. Each responds differently. Even for analysts who recognize the underlying unity of the actions of the first three states, the behaviors of $p$ and $q$ would probably appear to be distinct departures from the expected pattern. While the acts of nations $m$, $i$, and $j$ are consistent with the postulate that the decision makers in those nations perceive a security problem and are therefore undertaking policy initiatives to increase their defense capabilities or decrease their risks, $p$ and $q$ appear to behave quite differently. They do nothing at all; the conceptualization does not seem to apply.

The point to be noted, however, is that states $p$ and $q$ could be imbedded in the pattern if one began with the model in Table 2 and also knew the following: (1) The decision makers of $p$ view $m$ as a potential threat that upsets their $C$-to-$R$ ratio throughout the sequence, but they lack the capability to undertake any action to reset the inequality (see Postulates 1 and 5 in Table 2); (2) the decision makers of $q$ do not perceive the acts of any of the other states as threatening, perhaps because they are distant from or ideologically allied to them (see Postulate 3). Given these auxiliary conditions, one would expect the empirical behavioral patterns of $p$ and $q$ to appear distinct from those of states $m$, $i$, and $j$. While the latter three would be expected to increase their defense capacities or decrease their risks if the model were "true," $p$

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would remain inactive because its decision makers' willingness to act would be frustrated by lack of capability, while *q* would remain inactive because it simply is not motivated to act. The acts of all five states *m*, *i*, *j*, *p*, and *q* in Table 1 are in fact consistent with the postulate that decision makers are operating to establish favorable Capacity-to-Risk inequalities.[15]

These observations raise an important point that is generally overlooked: correlations and other such associations may not exist among empirical indicators simply because a given process is operating. If the security dilemma formulation shown in Table 2 were "true," states could reasonably be expected to behave differently. The relationships among the theoretical concepts could be general, but even if that were the case, the outcomes of the decision processes of different states *could be* rather heterogeneous. Again, if the model were "true," different empirical behaviors could follow.

## Problem No. 2:
### the overlooked possibilities of alternative triggers and "sometimes true" laws

The problem created by the potential for foreign policy substitutability is exacerbated by the practice among analysts of equating laws, theories, and models with universal—or at least highly generalizable—empirical "truths." At one level, this problem arises because the operation of even universal laws may not invariably be evident empirically. Scholars already recognize some of the reasons why this could be the case.[16] Our concern here is with a more fundamental—and generally overlooked—possibility. Put simply, analysts may be led astray when they associate

[15] Another reason why states *p* and *q* are of interest derives from the so-called "empty cell" problem. Instead of ignoring the "nonbehaviors" of actors (the zeros or empty cells in data matrices), analysts might attempt to utilize the important information conveyed by such nonbehaviors when they construct and evaluate their models. For an empirical example of this phenomenon in which "neutral" war behavior plays the key role in model structure and evaluation, see Dina A. Zinnes and others, "From War to War: A Stochastic Model of Alignment Behavior" (Paper presented at the Annual Meetings of the International Studies Association, Philadelphia, 1981).

[16] Even though a universal or "true" relationship may exist, for example, difficulties with indicator validity and reliability could mask that association at the empirical level. In our own earlier work, we have suggested that additional complications may develop as a result of faulty case selection procedures or the failure to specify the correct logical linkages among the concepts and indicators (see fn. 8). Those problems are critical. They raise doubts about the standard approach (see fn. 7). To the extent that such difficulties exist, or at least cannot be ruled out, the results of the tests become ambiguous. The lack of empirical support in a given analysis could indeed imply that a supposed law is not true, but it could also mean that operation of a true law is obscured by the procedures adopted by the researcher conducting the test.

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laws and theories with questions of universal truth or empirical generalizations because a given phenomenon may occur for a variety of distinct, totally incommensurable, reasons.

Just as the possibility of foreign policy substitution suggests that a given factor may lead to different results (a one-to-many mapping), the logical reverse is also imaginable. Different processes may lead to similar results; different factors may trigger similar responses (a many-to-one mapping). If policy makers can use different options in their pursuit of similar goals (i.e., substitute), they can also adopt identical options for different reasons, or employ similar means to pursue different goals.[17]

One aspect of the alternative trigger problem is implicit in part A of Table 1 above; in this guise, at least, the difficulty is easily circumvented by employing existing procedures. As we showed in that illustration, the *m*th state's increases in each round could plausibly be expected to lead to different responses by the *i*th and *j*th states; in a process of foreign policy substitution, a given trigger could lead to different empirical reactions. The point to be noted here is that even though the *m*th state elects to increase its defense spending in each round, we should not necessarily expect to find any single empirical trigger for the selection of that option. If we assume that the leaders of the *m*th state are concerned with their country's security situation, then increases in defense spending by *i* and *j*, the formation of an alliance between them, the dissolution of an alliance, efforts by *j* to import arms, and certainly attacks by the *j*th state should *all* be expected to contribute to decisions by leaders of the *m*th state to increase defense capabilities.

A second aspect of the alternative trigger problem is perhaps more important; in this form, the difficulty is less easily resolved. Let us imagine a world that comprises two sets of cases: those characterized by condition *A* and those that are not, i.e., a world in which there are

[17] Bruce Bueno de Mesquita appears to have exactly this problem in mind when he considers research on the outbreak of war and observes,

> ... the set of conditions sufficient for war may be so large that its specification is virtually impossible. ... Most efforts to find the cause or causes of war focus on environmental circumstances that compel policymakers to wage war, but if we attempt to show causal relationships between environments and war, we are forced to ignore the role of national leaders and to act as though nations were no more than automatically reacting mechanisms (Bueno de Mesquita, *The War Trap* [New Haven: Yale University Press, 1981], 4-5).

It is this line of argument—combined with his contention that decision makers are not mechanistically compelled—that leads Bueno de Mesquita to conclude that the search for the sufficient conditions for war should be abandoned in favor of a search for necessary conditions. A similar position is suggested by Singer (fn. 1). Other related points are contributed by James R. Kurth, "A Widening Gyre: The Logic of American Weapons Procurement," *Public Policy* 19 (Summer 1971), 373-404; and, Ludwig Von Bertalanffy, *General Systems Theory* (New York: Braziller, 1968).

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$A$ and $-A$ cases. Presume that type $A$ case are those in which decision makers are concerned with maintaining their states' national security, while decision makers in $-A$ polities pursue other goals. Imagine further that within the set of $A$ cases, the development of an external risk ($X$ or any $xi$) is invariably sufficient for an effort to resolve the security problem ($Y$), but external risk is not necessary for efforts to resolve the security problem because leaders are also concerned with through-time deteriorations in their defense capacities. Within the set of $A$ states, $Y$ follows wherever and whenever $X$ (or any $xi$) occurs, but $Y$ sometimes occurs in the absence of $X$ (or all $xi$). By contrast, when $A$ is not present, $X$ (and the $xi$) do not lead to $Y$, and $Y$ sometimes occurs in the absence of $X$ (or all $xi$).[18] Finally, let us make the usual presumptions regarding the validity and reliability of the indicators and consider the following possible case structure:[19]

| | | | | | | |
|---|---|---|---|---|---|---|
| Case 1 | $A$ | $x1$ | $-x2$ | $-x3$ | $X$ | $Y$ |
| Case 2 | $A$ | $-x1$ | $x2$ | $-x3$ | $X$ | $Y$ |
| Case 3 | $A$ | $-x1$ | $-x2$ | $x3$ | $X$ | $Y$ |
| Case 4 | $A$ | $x1$ | $x2$ | $x3$ | $X$ | $Y$ |
| Case 5 | $A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |
| Case 6 | $A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |
| Case 7 | $A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |
| Case 8 | $A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |
| Case 9 | $-A$ | $x1$ | $-x2$ | $-x3$ | $X$ | $-Y$ |
| Case 10 | $-A$ | $-x1$ | $x2$ | $-x3$ | $X$ | $-Y$ |
| Case 11 | $-A$ | $-x1$ | $-x2$ | $x3$ | $X$ | $-Y$ |
| Case 12 | $-A$ | $x1$ | $x2$ | $x3$ | $X$ | $-Y$ |
| Case 13 | $-A$ | $-x1$ | $-x2$ | $-x2$ | $-X$ | $Y$ |
| Case 14 | $-A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |
| Case 15 | $-A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |
| Case 16 | $-A$ | $-x1$ | $-x2$ | $-x3$ | $-X$ | $Y$ |

[18] Expressed simply, the hypothesis is of the form, "Given $A$, if $X$ (or any $xi$), then $Y$ (or any $yi$)"; i.e., given $A$, $X$ is sufficient, but not necessary, for $Y$. If $A$ were a measured variable rather than a theoretical postulate or axiom which is presumed to be either true or untrue, the proposition could be rewritten: "If $A$ and $X$ (or any $xi$), then $Y$ (or any $yi$); i.e., $A$ and $X$ are jointly sufficient for $Y$." Seen in this fashion, axioms are simply dichotomous variables that analysts either choose or are forced to leave unmeasured. This suggests that axioms should in general not be viewed as assertions of universal truth. They should instead be seen as antecedents in complex "if . . . , then . . ." statements which, in collapsed form, would read: "If the specified axioms are true for a given case, then the model should apply." Once this is seen, we can move quickly beyond debates over the universal truth or falsity of the assumptions themselves: the question of the truth or falsity of the model is put aside. We can even move beyond concerns for generality and become more directly concerned with (a) identifying cases in which the axioms are true and in which the model should therefore be useful, and (b) developing complementary models that hold under alternative sets of initial axioms.

[19] It is perhaps worth noting that this illustration entails an important simplifying as-

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Even for those analysts who recognize both the substitutability problem and the first type of alternative trigger problem and therefore elect to focus on $Y$ and $X$ rather than exclusively on the occurrence of any one of the $yi$ or $xi$, interpretation of the pattern would not be a simple matter if they ignored the $A/-A$ bifurcation of the set of cases and were concerned only with the $X/Y$ relationship. Put simply, application of the standard procedures would lead them either to reject the hypothesis that $X$ generally leads to $Y$ or to underestimate the importance of that linkage.

Within the domain circumscribed by the presence of $A$ (cases 1 to 8 above), however, $X$ is sufficient for $Y$. Outside that domain, $X$ does not lead to $Y$ (cases 9 to 16). The occurrences of $Y$ in the $-A$ domain must be triggered by something else. Knowing about $X$ or any $xi$ does not help.

That said, however, the point should be clear. While the $X/Y$ hypothesis is not general, it is nevertheless a perfectly sound—indeed, even "lawlike"— proposition within its domain of applicability (the cases in set $A$). Although ultimately we should like to develop an explanation that applies to all sixteen of the cases above, no additional work—either theoretical or empirical—is necessary to account for the first eight.

Thus, there are two different ways to approach the cases above. One is to develop and test hypotheses that are expected at the outset to be general or applicable to the universe of cases. Such a method would be consistent with current practices and the standard approach in which generalizations are found by aiming directly at them. Unfortunately, that strategy seems to reduce to an all-or-nothing situation in which we are forced to play to weakness; to explain completely any occurrence of a phenomenon, we must account for all occurrences.

The other strategy is to develop and test hypotheses that are expected to hold only under certain conditions (e.g., $A$ or $-A$; when decision makers seek to maintain their national security, and when they do not). Although we are ultimately interested in accounting for all occurrences of a phenomenon, the immediate (somewhat counterintuitive) strategy would entail an initial consideration of *what* relationships should hold ***when***, and a focus on only those cases in which the conditions pertain. Once those occurrences were explained, we could move on, perhaps developing very different models that would be applicable to the other

sumption: states have the opportunity to act whenever they are willing to do so. Under the unified actor/national security dilemma formulation outlined in Table 2, we would expect either increasing external risks or decreasing defense capacity to contribute to the leaders' willingness to resolve the security problem if the change in either factor is sufficient to reverse previously positive Capacity-to-Risk inequalities. Whether or not the decision makers would act on that disposition, however, would be affected by their objective opportunities to act.

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cases. This domain-specific theory approach would thus let us play to strength; rather than having to account for the phenomenon $Y$ both when decision makers are and are not concerned with their states' security dilemmas, we could conclude that there are occurrences that are explicable (i.e., those in the $A$ set) and others whose explanation requires more theoretical and empirical research (i.e., those in the $-A$ set). Rather than resting progress on the isolation of *one* explanation that holds for all, this approach allows us to move ahead by identifying *an* explanation that holds for some.

Let us consider now the unified actor/security dilemma formulation discussed above (see Table 2). Among other things, that model specifies that decision makers are motivated to establish the inequality expressed in Axiom 3: $Cnt > Rnt$. It suggests that decision makers want their states to be unconditionally viable; it would be consistent with their attempts to maintain at least local supremacy.[20]

If we presume for the purpose of argument that at least some decision makers in at least some settings do in fact key their decisions to that inequality, we come up against an obvious problem. With very little in the way of data analysis, we know that there are (or in principle, could be) states that act to increase their defense capabilities and decrease their risks at given times even though they have already so clearly satisfied the inequality and established their unconditional viability that the acts of other states do not reverse it. Because such states have already attained their goal, the motivation that presumably drives decision makers is lost; the antecedent condition in Postulate 4 (i.e., the condition $Cnt_1 < Rnt_1$) would simply never be established. Even though the model would therefore be adequate for accounting for the behaviors of states such as $m$, $i$, and $j$ in Table 1, it would leave totally unexplained the behaviors of states that are already convincingly viable.

Still assuming, then, that the formulation in Table 1 is useful for accounting for the decisions of some states, but also concluding that it could not logically be expected to account for the decisions of all polities, we are in a difficult situation if the goal is to discover a single, general formulation that applies to all cases both through time and across space. Even though the axioms of the model might be valid for some cases and the model's postulates might always hold within that domain, current practices would lead us to reject the model because neither it nor the relationships it implies are universally "true." Put more squarely, a model that might be perfectly adequate for explaining what happens in

[20] See Kenneth Boulding, *Conflict and Defense* (New York: Harper Torchbooks, 1962).

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*some well-defined* cases might be abandoned because it does not always apply or help to explain *all* cases.

The importance of this point becomes more apparent once we recognize that a variety of other motive assumptions could be postulated to characterize decision-making processes. One such possibility would be a variation on the national-security model in Table 2; rather than attempting to establish the $Cnt > Rnt$ inequality, decision makers could be posited as seeking the maintenance of the status quo so that the Capacity-to-Risk ratio at any time is at least equal to that of an earlier period. (Such a modified national-security formulation might be applicable to states that continue to increase their defense capacity and reduce the threat engendered by their opponents *even though* they are already unconditionally viable; it might be consistent with the illusory quest for perfect security or with the security dilemma of never knowing how much capability is enough.) In contrast to such national-security motivations, one could posit "domestic" viability models in which decision makers seek either to maintain enough political strength to keep from being deposed by their domestic opposition or to maintain at least their prior position vis-à-vis domestic threats. Other goals, and indeed combinations of several goals, could also be presented; any one of them could provide a rationale for expecting that decision makers might increase their military expenditures, create alliances, dissolve them, import arms, open negotiations, launch preemptive military strikes, and so on. Different processes, including a number that depart from an assumption of unified, value-maximizing decision makers, could lead to similar results.

A consideration of such alternative formulations suggests several important points. First, there is the partitioning role of axioms. Although there are probably some actual cases in which decision makers pursue almost any goal that one might reasonably wish to posit, there may also be instances in which decision makers are motivated by other concerns. To the extent that this seems plausible, such alternative motive forces or goals would not in any sense constitute the bases for rival or competing models. To be sure, one of the motive assumptions might be true for more cases than the others. If so, the formulation appropriate to that axiom would be more useful than the other models. The point, however, is that it would probably not be plausible to argue that any one of the motive axioms is always true. Thus, it would not be reasonable to expect any of the models to be universally applicable. We would therefore not be inclined to conduct a critical test in order to discover which is the more general. Instead, we would be interested in exploring whether or

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not a given model applies to a given, well-defined case or set of cases.

Second, if different processes do indeed lead to similar concrete empirical behaviors, the concept reflected by one such act could be different within different formulations. If the decision makers are dealing with a national viability or security dilemma, for example, an increase in their defense expenditures or the formation of an alliance would be interpretable as—and more importantly, serve as an indicator of—efforts to increase their state's defense capacity or to decrease the foreign risks with which they perceive it to be confronted; if they are seeking to resolve a simple governmental viability problem, on the other hand, those same acts would reflect efforts to increase their government's strength or to decrease the degree to which it is threatened by domestic forces. Even though analysts may tend to view acts as inherently or intrinsically falling on some fixed scale between conflict and cooperation, or between friendly and hostile behaviors, therefore, it might make more sense to interpret the actions of decision makers in the context of what they are attempting to accomplish.

Third, and perhaps most important, the argument suggests that it may be useful to expect that true social laws may not *always* hold, operate, or apply empirically. There may well be a variety of social laws, each of which is true, but which should be expected to hold only under certain—perhaps very special—conditions. Although it is possible that universal, always true, laws exist and we believe that scholars should continue their efforts to identify them, it is difficult to think of very many empirical universals that have been identified even by physical scientists. Thus, it may be useful to recognize that there could very well be laws that are in some sense "good," "domain-specific," or "nice," even though the relationships they imply are not necessarily empirically general.[21] Rather than assuming that there need be a single "always true" law that accounts for a given phenomenon whenever and wherever it has occurred or will occur, it may be more productive to think of several laws, each of which is always true under certain conditions (or within certain domains), but which is only sometimes true empirically because those conditions do not always hold in the empirical world. It might be more profitable to distinguish between the truthfulness of a relationship and the question of whether or not it holds in a given instance. Although the identification of universal relationships is still a

[21] Technically, it is not necessary to use words and phrases such as "domain-specific" or "nice" in connection with laws. They are used here only to emphasize our point that analysts may err if they necessarily equate laws with universals (see fn. 22 below). The adjective "nice" is used in similar fashion by G. Robert Boynton, "On Getting from Here to There: Reflections on Two Paradigms and Other Things," in Ostrom (fn. 12), 29-68.

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worthwhile goal, it may be useful to consider other research objectives that are equally important and perhaps more attainable.[22]

Ultimately, of course, analysts may develop several models, each of which may serve to explain a given phenomenon under different conditions. When that occurs—when we understand why *Y* occurs in *both* the *A* and $-A$ domains—it may be possible to integrate or synthesize those islands of understanding. In the interim, however, we should play to strength; we should learn not to reject or disregard an explanation that is only sometimes true.

## Some Consequences for the Study of International Relations

*Cumulation and theory.* In recent years, James N. Rosenau has become concerned with integrative cumulation and theory building in inter-

[22] Because the points depart so sharply from accepted arguments and assumptions, it may be worthwhile to consider the following observations from scholars who use somewhat different terms, but come to identical conclusions:

> There is a widespread misconception that theories are either "true" or "false." A number of examples in physical science stand in direct contradiction to this. . . . Each of these theories or models is tautologically true, *when* their postulates are fulfilled. . . . They are not theories to be confirmed or disconfirmed in general, but only confirmed or disconfirmed in specific applications. As a result, they are not theories which explain "how people behave"; they are theories or models which describe how people behaved in this or that circumstance . . . one fruitful line of development . . . will be not to ask what is *the* theory of a certain kind of behavior, or what are *the* postulates which correctly describe a general area of behavior. The tactic proposed here is to set about developing and applying a number of sometimes-true theories which relate consequences to postulates, and which may adequately describe behavior in a given situation (Coleman [fn. 6], 516-18; emphasis in original).

> . . . the laws of mathematics were invented too. The only demand made on them is that of consistency in a given context. *But in different contexts, different laws may operate.* Therefore, any statement of mathematics is valid or invalid not because certain relations are true or not true in the real world, but because these statements are or are not consequences of certain definitions and assumptions, which we are free to choose. The ambiguity of a mathematical statement results from a failure to specify with sufficient precision the exact context in which the statement is made (Anatol Rapoport, *Fights, Games and Debates* [Ann Arbor: University of Michigan Press, 1960], 297-98; emphasis added).

> Laws of nature . . . are different: to them the words "true," "probable" and the like seem to have no application. . . . [We could] adopt Snell's formula tentatively, hypothetically, as a guide to further experiments to see whether the phenomena always happen so. On this level, we might ask "Is Snell's *hypothesis* true or false?" meaning, "Have any limitations been found to the application of his formula?" But very soon—indeed as soon as its fruitfulness has been established—the formula in our hypothesis comes to be treated as a *law*, i.e., as something of which we ask not "Is it true?" but "When does it hold?" When this happens, it becomes part of the framework of optical theory, and is treated as a standard. Departures from the law and limitations on its scope . . . come to be spoken of as anomalies and thought of as things in need of explanation (Stephen Toulmin, *The Philosophy of Science: An Introduction* [New York: Harper Torchbooks, 1953], 78-79; emphasis in original).

Similar points have been made by Boynton (fns. 12 and 21), and Ostrom (fn. 12) and "Introduction: Making Sense Out of a Muddle," in Gillespie and Zinnes (fn. 12), 37-41.

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national relations. Perceived lack of progress in this area has prompted Dina Zinnes to inquire about *why* there has apparently been so little cumulation. Hopmann, Zinnes, and Singer have noted the need to assemble diverse pieces of evidence. Other analysts have considered the unconnected nature of the existing islands of theory and called for their integration.[23]

Our discussion has attempted to delineate a particular impediment to cumulation: *The general failure to conceptualize questions and indicators broadly enough to capture the relationships and processes that scholars are actually interested in studying*. We have argued that the islands of theory extant in the international relations literature have been too narrowly conceived for answering the questions that have been asked; we have suggested that analysts' ways of theorizing—and perhaps even their expectations about the results of such efforts—may have been inappropriate for dealing with the problems at hand. Analysts have lost their perspective, perhaps as a consequence of the abandonment of grand theory, their efforts to develop middle-range formulations, or their adoption of the even narrower hypothesis-testing procedures that were fostered by the empirical "third wave" of the 1960s and 1970s.

We have offered a number of specific recommendations about what should be done. Two remedies should be reemphasized. First, scholars should endeavor to merge the rigor and systematization of scientific inquiry with the broader, grand theoretical conceptualizations of the traditional literature in which substitutability and domain-specific laws were at least implicitly recognized. Second, analysts should begin to pay more careful attention to basic epistemological issues.[24] Nothing we have said casts doubt on existing research practices or suggests they should be abandoned. Our point is only that those practices, drawn as they are from other disciplines that are concerned with other types of problems, should be utilized with circumspection. Even though scholars are often impatient with epistemological issues and find themselves anxious to get on with their analyses, they should bear in mind that how they approach their problems—the manner in which they conceptualize them and the

[23] Rosenau, ed., *In Search of Global Patterns* (New York: The Free Press, 1973), Hopmann and others (fn. 11), and Zinnes (fn. 10). Scholars such as Singer and Russett have argued that analysts' collective research efforts are beginning to add up: J. David Singer (fn. 1), and Singer, "Tribal Sins on the QIP Reservation," in Rosenau (above), 167-73, and Bruce M. Russett, "International Interactions and International Processes: The State of the Discipline" (Paper presented at the Annual Meetings of the American Political Science Association, Denver, 1982).

[24] A similar point has been made recently in Bueno de Mesquita (fn. 17) and "Theories of International Conflict: An Analysis and an Appraisal," in Ted Robert Gurr, ed., Handbook of Political Conflict (New York: The Free Press, 1980), 361-98.

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methods they utilize in their attempts to solve them—will ultimately impinge on their results. If apparently distinct foreign policies could be responses to similar problems as a result of substitutability, and similar foreign policies could be responses to different problems as a result of the operation of domain-specific laws, then we may need to rethink the accepted procedures.

*Microlevel decisional analysis and process.* We have tried to do more than issue yet another call for theory, for more fruitful merging of tradition and science, or for greater attention to the implications of our methods. As in our other work, we have suggested here that a particular type of theoretical development might be especially useful. Following Theodore Abel, and using the work of the Sprouts as a starting point, both "opportunity" and "willingness" are posited as fundamental considerations.[25] Each state's objective opportunities circumscribe the range of possible acts and behaviors available to its decision makers. In the terms of Russett and Starr, the opportunities of states define their "menus for choice."[26] Because the factors that create or preclude different options are differentially distributed across states and even within states through time, such concerns are in some sense likely to be important for understanding their policy undertakings. A focus on such attributes could provide insights into the basic parameters or limits of the foreign policies of different countries.

To the extent that analysts are less interested in the parameters within which states operate and are ultimately more concerned with understanding why certain options are adopted at particular times, however, our contention is that a focus on the attributes of states will not suffice. In this, we agree with Waltz that

> ... different states have produced similar as well as different outcomes, and similar states have produced different as well as similar outcomes. The same causes sometimes lead to different effects, and the same effects sometimes follow from different causes.[27]

Expressed in our terms, it appears that a focus on attributes could tell us what states can do, and therefore possibly what the aggregate probabilities are that certain types of events will occur in a given group or sample of states. A focus on attributes cannot tell us, however, which particular states will do what, or when they will do it. Certain objective

[25] Abel, "The Element of Decision in the Pattern of War," *American Sociological Review* 6 (December 1941), 853-59; Sprout and Sprout (fn. 13).

[26] Bruce Russett and Harvey Starr, *World Politics: The Menu For Choice* (San Francisco: W. H. Freeman, 1981).

[27] Waltz (fn. 1), 37.

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capabilities may be *necessary* for a foreign policy initiation, but they should not be expected to be *sufficient* for that undertaking—unless, that is, decision makers are homogeneously willing to avail themselves of all their opportunities. The problem is that it does not seem reasonable to expect all foreign policy makers in all states to pursue identical goals.[28]

It is the recognition of this problem that leads Waltz to reject "reductionist" or "analytic" theories focused on the individual, decisional, or national levels of analysis, and to argue instead for a systemic approach.[29] While we agree with Waltz and other systems-level theorists on the problem, and also conclude that static analyses of the attributes of states will not bring progress, our own solution is quite different. We argue that, while the objective capabilities of states may define the range of possibilities, progress can be made by understanding the *processes* by which decision makers choose certain options over others. This suggests that we might usefully follow Bueno de Mesquita's expected utility approach[30] and study how decision makers select from among varying sets of available, potentially substitutable alternatives, paying particular attention to the cognitive or perceptual processes that we couch in terms of "willingness," and that the Sprouts cast in terms of "cognitive behaviorism."[31] Put differently, we need to consider how the willingness and opportunities of states interact to produce foreign policy behaviors.

[28] Singer, himself a long-time proponent of the systemic level approach and the view that foreign policy decision makers may at least initially be presumed to be rather homogeneous, comes close to this point in reviewing the realpolitik, arms race, power transition, economic development, and imperialism models:

> ... at rock bottom the most important difference amongst the contending causes of war models is that of the foreign policy decision process. That is, each model assumes—often implicitly—a different class of decision makers in power and each postulates a different set of decision rules. ... Note that we have assumed, to this juncture, a high degree of homogeneity in decision makers and the rules they employ, but *to move closer to a full explanation, that assumption would have to be relaxed* (Singer [fn. 1], 14-15, emphasis added).

See also J. David Singer, "The Level-of-Analysis Problem in International Relations," in Rosenau (fn. 13), 20-29.

[29] Waltz (fn. 1), esp. pp. 37 and 65.

[30] Bueno de Mesquita (fn. 17).

[31] One critique of Waltz (fn. 1) is that he has underestimated the cognitive links between social entities and their environments. The environment—the international system—is important. Types of international systems could (but need not) be conceptualized as operating to define domains such as the $A$ and $-A$ contexts dicussed in our example in the previous section. Nevertheless, Waltz artificially separates the "entity" and the "environment," using the Sprouts' terms for the "ecological triad" (Sprout and Sprout, fn. 13). He seems to ignore the third leg of that triad, the entity-environment relationship, which *must* have a cognitive component. The "enduring anarchic character of international politics" (fn. 1, p. 66), which he regards as the central factor in the analysis, *cannot* explain the variance in behavior.

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# *Review Articles*

# WAR AND EXPECTED-UTILITY THEORY

By R. HARRISON WAGNER*

Bruce Bueno de Mesquita, *The War Trap*. New Haven: Yale University Press, 1981, 238 pp.

"FOR too long," states Bruce Bueno de Mesquita, "the divorce of explicit, rigorously derived theory from explicit, rigorously derived evidence has hampered the study of international conflict" (p. ix). As a result, he says, previous scholarship has not led to generally accepted conclusions:

> Nearly twenty-five centuries after Thucydides wrote *The History of the Peloponnesian War*, we continue to debate whether a "balance of power" is good or bad, whether alliances are entangling or liberating, whether great powers behave differently from lesser powers, whether decision-making patterns vary markedly or are essentially the same in different centuries or in different cultures (p. x).

In *The War Trap*, Bueno de Mesquita claims to offer for the first time a deductive theory of international conflict that settles these issues:

> The expected-utility theory suggested here purports to answer all of these questions while also providing insights into other old issues and some new ones (p. x).

The conclusiveness of this work, in contrast to preceding works, is supposed to be the result of subjecting a deductively derived theory to a systematic empirical test. Many specific predictions are apparently rigorously derived from the assumption that foreign policy makers attempt to maximize their expected utility. The theory is tested by comparing its predictions with carefully controlled observations of all wars fought between 1816 and 1974 (as well as a large number of other international conflicts). The results seem to constitute an extremely impressive confirmation of the theory.

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These are strong claims, and they merit careful scrutiny. Unfortunately, I do not believe they are fully justified. In the first part of this article, I will argue that Bueno de Mesquita's theory cannot be derived from his assumptions. In the second part, I will examine the operational version of his theory, which is the one that was actually tested. I will argue that it can be more plausibly interpreted as a version of a theory he professes to discredit rather than the theory he professes to deduce from his own assumptions. I will also argue that the empirical evidence offered provides less impressive confirmation even of the theory actually tested than Bueno de Mesquita claims.

## The Theory

Bueno de Mesquita assumes that foreign policy decisions are made by individual political leaders who are rational and self-interested, and whose interests are affected by the outcomes of wars. Since these decisions are made under uncertainty, he assumes that leaders act so as to maximize expected utility. Although Bueno de Mesquita defends the plausibility of these assumptions (pp. 12-36), he believes that they must ultimately be justified by whether the predictions they imply are true or false (p. 28). Thus I will not comment on his justification of them. In his discussion of the plausibility of one of them, however, an ambiguity is introduced that must be resolved if we are to understand the theory.

The author draws a distinction between a dictatorial leader and one whose consent is necessary for any alternative to be chosen. He says he assumes the latter:

> This means not that we are assuming every state is ruled by an iron-fisted, coercive dictator but simply that a single individual is the critical gatekeeper who might turn the nation away from initiating a war (p. 27).

This situation seems to imply the presence of a leader who has a veto over any foreign policy decision, but who cannot alone decide what is to be done. Stated more precisely, no decision can be made without the leader's consent; and if the leader is unable to obtain the consent of a sufficient number of other decision makers for some alternative to the status quo, then the status quo will be chosen (see also p. 26).

This assumption is weaker (and therefore more plausible) than that a single leader makes foreign policy in accordance with his preferences. Since it is weaker, however, it fails to imply that foreign policy decisions maximize the leader's utility. For it is possible that the only decision preferred to the status quo by both the leader and others whose support is necessary is one that is lower on the leader's schedule of preferences

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than any alternative but the status quo. In particular, it would be consistent with this assumption for a leader to agree to initiate a war with another country even though he preferred a diplomatic initiative—an outcome that might occur if all other decision makers preferred war to any other option and the leader preferred war to doing nothing at all.

Elsewhere in the book, Bueno de Mesquita seems to make a stronger claim for his theory. For example:

> ... I assume ... the leader is guided by a desire to maximize the net benefits he expects from his foreign policy choices. A policymaker will never choose an action that is expected to produce less value—or utility—than some alternative policy (p. 29).

Toward the end of the book, he states that "leaders expecting a larger net gain through diplomacy than through war ... should rationally elect to pursue their goals through diplomatic bargaining and negotiating" (p. 183). But we have seen that these statements are not implied by his assumption about the leader's authority.

The proposition that Bueno de Mesquita wishes to test is that no state will go to war if the expected utility of doing so is less than the expected utility of continuing current policy. This is implied both by the proposition that strong leaders have a veto over foreign policy decisions and the proposition that strong leaders make foreign policy in accordance with their preferences. Many statements in the book seem to assert that evidence for the truth of the proposition Bueno de Mesquita wants to test would confirm the theory that foreign policy decisions maximize someone's expected utility, but this claim is too strong.

Bueno de Mesquita's justification for concentrating on this narrow hypothesis is somewhat confusing. For example:

> The choice of war or peace depends on the choices of individuals and not on compulsion by circumstance. Their choices depend on their estimation of costs and benefits and their comprehension of right and wrong.... We cannot provide a general framework of moral values that reveals what decision makers believe to be right or wrong, but we may be able to capture some essential characteristics of their assessment of the expected costs and benefits associated with particular war-or-peace situations. It is in those calculations that necessary conditions for war are to be found (pp. 5-6).

Later in the book, he notes that

> ... we should bear in mind that many idiosyncratic, and even irrational, elements may enter into war-and-peace decisions. Indeed, even such additional rational calculations as the expected utility of diplomacy or the expected long-term political-strategic rather than policy-based costs of war

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> enter such decisions, although they are not explicitly incorporated in the theory developed in the next chapter. The presumed importance of the theory I will set forth concerns the idea that whatever other factors may encourage leaders away from or toward war, the conditions stipulated in the theory *must* be satisfied for rational leaders to initiate war (p. 45; emphasis in original).

But if leaders are irrational or motivated by moral considerations, or if there are other factors in their utility functions than those incorporated in Bueno de Mesquita's model, it is not necessary that the expected utility of war (measured without these other factors) be greater than the expected utility of continuing current policies, for war to be chosen. And if the leader only has a veto over foreign policy decisions, our knowing his evaluation of other alternatives would allow us to say no more than we can say without that knowledge. Indeed, if Bueno de Mesquita assumes that decision makers only have a veto over foreign policy decisions, the most straightforward justification for focusing on this narrow hypothesis is that no stronger hypothesis can be inferred from that assumption.

### Bilateral War

Let us now consider how leaders are assumed to determine the expected value of war, beginning (as Bueno de Mesquita does) with a war in which only the aggressor and the defender are expected to fight. The author assumes (implausibly) that there are only two possible outcomes to any war: either the aggressor wins or the defender wins. Thus, if the Soviet Union had driven Hitler's forces out of its territory and restored the boundaries of 1939, he assumes that to Hitler, this would have been equivalent in value to the outcome that actually occurred. His hypothesis is that state $i$ will not attack state $j$ if the expected value of doing so is less than the expected value of maintaining current policies. When there are only two possible outcomes, this condition can be expressed as:

$$p_i W_i + (1 - p_i) L_i \geqq S_i \qquad (1)$$

where $p_i$ is the probability that $i$ will defeat $j$, $W_i$ the value to $i$ of winning, $L_i$ the value of losing, and $S_i$ the value of whatever happens if $i$ maintains current policies (which may also, of course, be uncertain).

Bueno de Mesquita wants to state the hypothesis as saying that $i$ will not attack if the expected utility of doing so is less than zero. In evaluating his formulation, it is important to bear in mind that there is no natural zero point for a utility function. One can assign the value of zero to one of the three possible outcomes; but if it is not the worst possible

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outcome, there is no reason to expect that both the others will be bounded by any particular numerical values. Bueno de Mesquita's discussion assumes otherwise.

His formulation can be derived from (1) by subtracting the value of $S_i$ from all the utility values in it, in which case (1) becomes:

$$p_i(W_i - S_i) + (1 - p_i)(L_i - S_i) \geqq 0 \qquad (2)$$

The author maintains that the value of a war to $i$ is based on the value $i$ places on its own "policies" as compared to the "policies" of $j$ (pp. 46-47). (That is literally all he says about what is being evaluated.) Victory means that $i$ imposes its policies on $j$; defeat means that $j$ imposes its policies on $i$. The most, he claims, that $i$ could gain from war with $j$ is the difference between the value it places on its own policies ($U_{ii}$) and the value it places on $j$'s ($U_{ij}$). This implies that the value $i$ receives in the absence of war ($S_i$) must be $U_{ij}$, and ($W_i - S_i$) becomes ($U_{ii} - U_{ij}$).

Next, Bueno de Mesquita argues that the most $i$ could lose from a war is also the difference between the value $i$ places on its own policies and the value it places on $j$'s. But this implies that the value $i$ receives in the absence of war is $U_{ii}$, which contradicts what Bueno de Mesquita says about the potential gains from war. Yet he does indeed equate ($L_i - S_i$) with ($U_{ij} - U_{ii}$) (p. 47), so that (2) becomes:

$$p_i(U_{ii} - U_{ij}) + (1 - p_i)(U_{ij} - U_{ii}) \geqq 0 \qquad (3)$$

A comparison of (2) and (3) shows the contradiction: $U_{ij}$ is equated with both $S_i$ and $L_i$, and the alternative to war is considered to be both $U_{ij}$ and $U_{ii}$.

From these contradictory assumptions, Bueno de Mesquita infers that the possble gain in utility from winning a war is necessarily equal to the possible loss in utility from losing it—which implies that the expected value of war is positive if, and only if, the probability of winning is greater than the probability of losing. Thus he derives an implausible conclusion from inconsistent assumptions.

Moreover, his discussion seems to imply that $S_i$ can be the value $i$ places on the status quo. But this will not do. The consequence of $i$'s not attacking $j$ might be, not the status quo, but an attack by $j$ on $i$. If $i$ expected to do better by attacking first (even though either outcome would be worse than the status quo) then $i$ would rationally initiate a war with $j$ even though the expected utility of doing so was less than the utility assigned to the status quo. Bueno de Mesquita argues that in that case $j$'s expected utility would be positive, and $j$ would be the initiator (p. 66). But if $i$ attacks $j$, $i$ is obviously the initiator, and would so appear

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in the data the author uses to test his theory. Moreover, it is in any case not necessary that $j$ actually prefer war to the status quo for $i$ to choose to attack: $i$ need only think it sufficiently likely that $j$ would prefer to attack in order to make an attack of its own seem preferable to the alternative.

But the outcome of a war may also be influenced by the participation of allies of either the aggressor or the defender. Further problems with the theory emerge when we consider how Bueno de Mesquita introduces this possibility into the expected-utility calculation.

### The Role of Allies

Suppose that, in addition to $i$ and $j$, there is also a third country, $k$. Now $p_i$ will be influenced by whether $k$ stays neutral, fights as an ally of $i$, or fights as an ally of $j$. Thus, $p_i$ must be replaced by three conditional probabilities: $p_{ik_i}$, $p_{ik_j}$, and $p_{ik_n}$, meaning "the probability that $i$ will defeat $j$ if $k$ fights as an ally of $i$," "the probability that $i$ will defeat $j$ if $k$ fights as an ally of $j$," and "the probability that $i$ will defeat $j$ if $k$ remains neutral." The last is plainly equivalent to $p_i$ in the earlier calculation, but the others are not.

In order for the leader of $i$ to calculate the expected utility of a war with $j$, one more piece of information is required: the probability with which $k$ will play each of these three roles. Let us call the latter $p_{k_i}$, $p_{k_j}$, and $p_{k_n}$ (standing for the probability that $k$ will ally with $i$, that $k$ will ally with $j$, and that $k$ will remain neutral). Then $i$ must multiply the probability that $k$ will align with $i$ by the expected value if it does so, and do the same for the other two possibilities; it must then add up the results to determine the overall expected utility of a war with $j$. Using Bueno de Mesquita's inconsistent assumptions about the values of winning and losing, this can be stated equivalently as:

$$P(U_{ii} - U_{ij}) + (1 - P)(U_{ij} - U_{ii}) \tag{4}$$

where $P = (p_{k_i}p_{ik_i} + p_{k_j}p_{ik_j} + p_{k_n}p_{ik_n})$. If there is a fourth country, $l$, the problem becomes more complicated, since then one must determine the expected value to $i$ with each possible combination of roles by $k$ and $l$. This, at any rate, is what the notion of expected utility would imply. What Bueno de Mesquita does with the problem is a different matter.

First, he ignores the need to estimate the probability that $k$ will play each of the three possible roles. Second, he treats each possible ally separately, and makes an expected-utility calculation for $i$ if the ally fights on the side of $i$, and if the ally fights on the side of $j$. Thus, in considering the probability that $i$ and $k$ might defeat $j$, he ignores the

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possibility that $l$ might be allied with $j$. Third, he adds up all the expected-utility values for $i$ with each third country assumed to be allied with $i$, and he adds up all the expected-utility values for $i$ with each third country assumed to be allied with $j$. Then he *subtracts* this second sum from the first sum. This, he argues, gives him "the *net* expected value from the support decisions of all third parties" (p. 57; emphasis in original)—which, when added to the expected value of a bilateral war, results in $i$'s overall expected utility.

But this procedure is inconsistent with the concept of expected utility. To make the problem clear, let us look at a simpler example. Consider the decision to invade Normandy during World War II. In good weather the Allies had a greater chance of succeeding than in bad weather. How should they calculate the expected value of an attack? Clearly they should estimate the probability of good or bad weather. Bueno de Mesquita's procedure implies that they should subtract the expected value of attacking in bad weather from the expected value of attacking in good weather, in order to find the 'net' expected value of attacking. But this is exactly equivalent to calculating the expected value of attacking while assuming that the probability of good weather is 1 and the probability of bad weather is $-1$.

There are also problems with the two expected-utility equations that Bueno de Mesquita uses to consider $i$'s expectations when $k$ allies with it, and when $k$ allies with $j$. Instead of a version of equation (3) above with the appropriate conditional probabilities, we find, for example, the following (omitting some components that deal with possible future changes in utility values)(p. 56):[1]

$$E(U_i)_k = p_{ik}U_{iki} + (1 - p_{ik})U_{ikj} \qquad (5)$$

This is supposed to represent the expected value to $i$ of a war with $j$ if $k$ fights on the side of $i$. The appropriate conditional probability of $i$'s winning is $p_{ik}$. But what are the other terms? They *should* be the utility to $i$ from defeating $j$ and the utility to $i$ from losing to $j$. But they are not. According to Bueno de Mesquita, $U_{iki}$ is "$i$'s perception of the utility to be derived from [$k$]" (p. 56). It is difficult to see what this can mean.

The word "utility" in expected-utility theory means a measure of preference over outcomes. So far, the only possible outcomes that have been introduced are winning and losing a war against $j$. In his discussion of bilateral wars, as we have seen, Bueno de Mesquita argues that what

[1] There are also serious problems in interpreting the way in which these possible future changes in values are incorporated in $i$'s expected-utility calculations, but I will omit discussion of them since the difficulties with the main part of the analysis are already fundamental.

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is at stake in a war is whether $i$ will compel $j$ to adopt $i$'s preferred policies, or whether $j$ will compel $i$ to adopt $j$'s preferred policies. How is $i$ now to derive utility from $k$—by defeating it and compelling it to adopt $i$'s policies? After all, $k$ is $i$'s ally.

Here is the author's explanation:

> Some allies may contribute soldiers, equipment, and supplies to a war, while others may offer little more than moral support. The problem for a leader considering the initiation of a war is to estimate how much value, or utility, each other state is expected to contribute to the victory (or defeat) of the initiating or opposing nation. Presumably, those that expect to be significantly affected by the outcome of the war will be inclined to aid their "ally," while those anticipating only minor consequences from the war will be less inclined to become involved (p. 56).

Thus, $U_{ikj}$ is supposed to represent the utility to $i$ of $k$'s contribution to the war. But the significance of $k$'s contribution is *completely* captured by its effect on the probability that $i$ will win the war, and is thus included in the conditional probability term.

Let us consider now the second component in the sum represented in (5) above. $(1 - p_{ik})$ is the probability that $i$ will lose to $j$, assuming that $k$ aids $i$. $U_{ikj}$ is "$i$'s perception of the utility to be derived by $j$ from [$k$]" (p. 56). Thus, presumably, if $i$ loses the war even though $k$ has fought as $i$'s ally, $j$'s utility will be affected by $k$, but otherwise it will not. It is not clear what empirical interpretation can be given to this assumption. More importantly, what is a measure of $j$'s utility doing in $i$'s expected-utility calculation?

For every third country $k$, there is an expected-utility calculation identical to (5); all of them are then added together. There are similar calculations based on the assumption that each $k$ allies with $j$. Then the sum of these second calculations is subtracted from the sum of the first, and the result is added to the expected utility of a war between $i$ and $j$ alone. This is supposed to result in $i$'s overall expected utility from a war with $j$. Bueno de Mesquita's hypothesis is that $i$ will not initiate a war with $j$ if this number is less than zero. He believes that this follows from his assumption that political leaders maximize their expected utility, but it plainly does not.

### Attitudes Toward Risk

This hypothesis is subjected to one further refinement before it is tested. Leaders, the author states, may differ in their attitudes toward risk; and thus not every leader will follow this expected-utility calculation exactly. Leaders may be either risk-averse or risk-acceptant. And they

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may or may not be confident of their estimates of the components of the expected-utility calculations—they may decide under conditions of risk or under conditions of uncertainty (pp. 33-40, 59-64). The discussion of these possibilities is complex. Since I argue that Bueno de Mesquita's theory is really not an expected-utility theory at all, I will not review it in detail. But since it is relevant to an examination of the test of his hypothesis, we must at least pay attention to its broad outlines.

In this context, Bueno de Mesquita makes an additional implausible assumption: that leaders are never uncertain of the probabilities of winning and losing wars, or of the value of winning or losing them, but are only uncertain about the "utility" to be derived from their allies (pp. 36-40, 61). He then assumes that under uncertainty (rather than risk), risk-acceptant and risk-averse leaders differ in the way they calculate the "utility" to be derived from third countries. Briefly, a risk-averse leader of $i$ will assume that countries thought to prefer $j$ over $i$ will perhaps aid $j$, whereas those who prefer $i$ over $j$ will definitely not aid $i$. A risk-acceptant leader, on the other hand, will not ignore the possibility of aid from his allies. (The precise decision rules assumed are summarized on p. 63.)

There are criticisms that could be made of Bueno de Mesquita's treatment of attitudes toward risk, as well as of his attempt to measure leaders' utilities for winning and losing wars. A more basic problem, however, is the fact that the entire theory cannot be derived from Bueno de Mesquita's assumption that foreign policy makers attempt to maximize expected utility. At the beginning of the book, he deplores the fact that empirical studies have tended rigorously to test *ad hoc* hypotheses whose precise meaning and theoretical foundation have been too little examined. Although the theory in *The War Trap* is supposed to correct this deficiency, it does not: since it cannot be derived from the expected-utility hypothesis, it appears to be entirely *ad hoc*.

This conclusion leads to an interesting puzzle: how could a hypothesis with so little foundation be so impressively confirmed by the evidence Bueno de Mesquita marshals in its behalf? He asserts:

> I expect that all tests of the theory will yield results so strong that there can be no question of their substantive significance. As a minimal requirement, each deduction from the theory must be supported by evidence falling in the direction predicted and (when appropriate) must be statistically significant at least at the .025 level. Furthermore, I expect measures of one-way association (that reflect necessary, but not sufficient, conditions) to be as strong as .70 or more. Results weaker than that, though still potentially of interest, cast doubts on the general explanatory power of the theory (p. 126).

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How could a theory that seems so arbitrary possibly meet such requirements?

## The Test

If one is not satisfied that Bueno de Mesquita was just extraordinarily lucky in finding evidence to confirm his theory, two other possible explanations must be considered: that the theory can be given a different and more plausible interpretation than the one he offers, or that the empirical test is in fact not as impressive as it appears. In fact, both these propositions are true.

Suppose I wanted to test the hypothesis that no state will initiate a war if its military capabilities, together with those of its allies, are less than those of its opponent and its allies. Using Bueno de Mesquita's measure of capabilities, let us call the state considering a war $i$, its prospective opponent $j$, $i$'s allies $k_1, k_2, \ldots k_n$, and $j$'s allies $l_1, l_2, \ldots l_m$. Then the hypothesis would be that a necessary condition for $i$ to start a war with $j$ is that

$$cap_i + \Sigma cap_k \geqq cap_j + \Sigma cap_l. \tag{6}$$

This condition can be expressed equivalently as

$$(cap_i - cap_j) + \Sigma \mathrm{cap}_k - \Sigma cap_l \geqq 0 \tag{7}$$

But this formulation assumes that it is certain that all $k$'s will fight for $i$ and all $l$'s for $j$. If we wanted to take into account uncertainty about which third countries will fight for whom, we could weight the capabilities of potential allies by the probability of their fighting. If we assume that a potential ally of $i$ will either fight for $i$ or stay neutral, but not fight for $j$ (and similarly for $j$'s potential allies), then (7) becomes

$$(cap_i - cap_j) + \Sigma \mathrm{p}_k \mathrm{cap}_k - \Sigma \mathrm{p}_l \mathrm{cap}_l \geqq 0 \tag{8}$$

where $p_k$ is the probability that some potential ally $k$ will fight for $i$ and similarly for $p_l$. We can divide both sides of the inequality in (8) by $(cap_i + cap_j)$, yielding

$$\frac{cap_i - cap_j}{cap_i + cap_j} + \sum_k \left( p_k \cdot \frac{cap_k}{cap_i + cap_j} \right) - \sum_l \left( p_l \cdot \frac{cap_l}{cap_i + cap_j} \right) \geqq 0 \tag{9}$$

Let us ignore for the moment the problem of estimating $p_k$ and $p_l$, and consider how to test this hypothesis. The hypothesis states that (9) is a necessary condition for $i$ to initiate a war. We cannot be certain, however, that the leaders of $i$ measure capabilities in the same way as

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Bueno de Mesquita does. Thus, if the condition is often but not always satisfied, it will be impossible to distinguish between two possible explanations of that fact:

*Explanation 1*: Our hypothesis is true, but our measure of capabilities is not identical to the one leaders use.

*Explanation 2*: If (9) is not satisfied, it is *unlikely* that a nation will initiate war, but not impossible that it will do so.

A third possible explanation of our findings would be that decisions to go to war are unrelated to (9), and thus the frequency of observations in accordance with (9) is simply the result of chance. But we can determine the probability of getting such observations as the result of chance; if it is very low, we can reject that explanation in favor of the other two.

It is obvious that (9) must be true of one or the other of two states at war, or (if their capabilities are equal) of both. If the initiation of war is entirely unrelated to (9), then in all those wars in which the capabilities of the aggressor and the defender are not equal, it must be equally likely that the aggressor or the defender will satisfy (9). For those wars, then, we would expect (9) to be true of 50 percent of the aggressors even if there is no relation between the condition it states and the decision to initiate a war.

I have developed this example at some length because the hypothesis that Bueno de Mesquita actually tested is very close to it; what has just been said about testing it applies to his test of his hypothesis as well.

## Bueno de Mesquita's Hypothesis

We will need to use a small amount of algebra and consider carefully how Bueno de Mesquita has operationalized his basic hypothesis. According to *The War Trap*, the expected utility to $i$ of a war with $j$ is the sum of two factors: the expected utility of a bilateral war with $j$, and the "net" expected utility resulting from the decisions of all third countries. If we continue to ignore the terms the author uses to represent possible changes in utility over time (which he also apparently does in testing his theory), then the first factor in $i$'s expected utility is

$$E(U_i)_b = p_i(1 - U_{ij}) + (1 - p_i)(U_{ij} - 1) \qquad (10)$$

where $U_{ij}$ is a measure (Kendall's tau $b$) of the similarity of $i$'s and $j$'s alliance commitments, and $E(U_i)_b$ stands for $i$'s expected utility in a bilateral war with $j$. Equation (10) is easily reduced to

$$E(U_i)_b = (2p_i - 1)(1 - U_{ij}). \qquad (11)$$

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But $p_i$ (the probability that $i$ will defeat $j$) is represented by the proportion of the total capabilities of $i$ and $j$ possessed by $i$, or $(cap_i)/(cap_i + cap_j)$. Thus $(2p_i - 1)$ is equivalent to $(cap_i - cap_j)/(cap_i + cap_j)$, and (11) becomes

$$E(U_i)_b = (1 - U_{ij}) \frac{cap_i - cap_j}{cap_i + cap_j} \tag{12}$$

(These values for the capabilities of $i$ and $j$ are subject to further adjustments that I will discuss shortly.)

The second factor in $i$'s expected utility (the "net" expected utility from the support decisions of third parties) is represented (on p. 58) by

$$E(U_i)_k = \sum_k [(p_{ik} + p_{jk} - 1)(U_{iki} - U_{ikj})] \tag{13}$$

where $E(U_i)_k$ represents $i$'s expected utility from a third country $k$, $U_{iki}$ represents a measure of the similarity between $i$'s and $k$'s alliances, $U_{ikj}$ represents a measure of the similarity between $k$'s and $j$'s alliances, $p_{ik}$ represents the probability that $i$ will win against $j$ if $k$ fights on the side of $i$, and $p_{jk}$ represents the probability that $j$ will win if $k$ fights on the side of $j$. As before, probabilities of winning are represented by proportional capabilities. If we substitute the relevant values into (13), we get

$$E(U_i)_k = \sum_k \left[ \frac{cap_k}{cap_i + cap_j + cap_k} (U_{iki} - U_{ikj}) \right]. \tag{14}$$

The overall expected utility of $i$ in a war with $j$ (according to Bueno de Mesquita) is the sum of (12) and (14). Adding them and rearranging the result slightly provides us with the hypothesis actually tested, which is that $i$ will not initiate a war with $j$ unless

$$E(U_i) = \frac{cap_i - cap_j}{cap_i + cap_j} + \sum_k \left( \frac{U_{iki} - U_{ikj}}{1 - U_{ij}} \cdot \frac{cap_k}{cap_i + cap_j + cap_k} \right) \geqq 0 \tag{15}$$

Let us now compare the hypothesis stated in (15) with the hypothesis stated in (9). If the first fraction after the plus sign in (15) can be plausibly interpreted as a substitute for the probability terms in (9), then the two conditions are obviously quite similar.

It is easy to show that it can be so interpreted. Remember that $U_{iki}$ is an index of the similarity of $k$'s and $i$'s alliance commitments, while $U_{ikj}$ is an index of the similarity of $k$'s and $j$'s alliance commitments. The value of this index can range from 1 to $-1$. If $k$ is closer to $i$ than to $j$ in its alliance commitments, then this fraction will be positive. If $k$ is closer to $j$ than to $i$, the fraction will be negative, which means that

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the plus sign in (15) must be replaced by a minus sign. Thus, a plausible interpretation of this fraction is that if $k$ is closer in its commitments to $i$ than to $j$, it will fight on the side of $i$ or remain neutral, but not fight on the side of $j$; the probability of its aiding $i$ is estimated by this fraction. If $k$'s commitments are closer to $j$'s than to $i$'s, then it is treated as a potential ally of $j$'s [i.e., as an $l$ in (9)]; the probability of its aiding $j$ is determined in the same way.

All the $U$'s in this fraction stand for Kendall's tau $b$. It can be shown that, without ties, the fraction has a straightforward interpretation: it represents the number of pairs of countries ranked similarly by $k$ and $i$ but differently by $k$ and $j$, minus the number ranked similarly by $k$ and $j$ but differently by $k$ and $i$, divided by the total number of pairs that could be ranked similarly by one pair of countries but differently by the other. There are many ties in Bueno de Mesquita's data (see the examples he gives on pp. 116-17); but even so, the intuitive meaning remains the same. Thus, the fraction behaves roughly like a probability measure, varying between 0 and 1. (When the denominator is zero, the numerator must be zero as well.) Altfeld and Bueno de Mesquita have presented evidence that the direction of its variation (determined by the numerator) is empirically plausible.[2]

The *only* difference, then, between (15) and (9) is that in (15) the percentages representing the capabilities of the principals and their potential allies have different bases. But this *ad hoc* feature of (15) may actually improve its accuracy. The larger $cap_k$ is in relation to $cap_i$ and $cap_j$, the more important any potential contribution from $k$ will be in determining the outcome of a war. But there is likely to be a lot of error in our estimate of the role of allies' contributions in the balance of capabilities between $i$ and $j$. Thus, the more important that contribution is in determining whether the hypothesized condition is fulfilled, the more likely it is that our estimate of it may be mistaken. The effect of the way allies' capabilities are counted in (15) is to dampen down their importance the larger they are in relation to the capabilities of $i$ and $j$. In effect, leaders are expected to be more cautious of relying on allies' contributions as these contributions become more significant relative to their own. Conceivably this condition makes (15) a more plausible statement of the hypothesis than if it were more similar to (9). But, since the null hypothesis is that the initiation of wars is *unrelated* to (15), this feature of (15) may not be very significant one way or the other.

[2] Michael Altfeld and Bruce Bueno de Mesquita, "Choosing Sides in Wars," *International Studies Quarterly* 23 (March 1979), 87-112.

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Before considering Bueno de Mesquita's test of his hypothesis, we must note two refinements to which he subjects the inequality in (15). First, the capability values are modified to allow for the geographic distance between $i$ and $j$. Thus, for example, $cap_i$ in (15) is reduced to allow for the fact that $i$ must fight $j$ at some distance from $i$. And the value of $cap_k$ is reduced to allow for the distance from $k$ to $i$ or $k$ to $j$, whichever leaves $k$ with the greater proportional contribution.

Second, as we have already seen, the author assumes that leaders may vary in their attitudes toward risk and in the amount of confidence they have in their estimates of the likelihood of allies' contributions. Thus, some leaders may not count some $k$'s, while others may count all of them.

These modifications are important for the following reason: if (15) is evaluated *without* them, then—like (9)—for every pair of countries $i$ and $j$, (15) must be satisfied by one or both—indeed, $E(U_i) = -E(U_j)$. This is obvious, since $U_{ij}$ is the same for both $i$ and $j$, as are the values for $k$'s proportional contribution; thus, the inequality for $j$ is derived simply by reversing the values for $i$ and $j$ in (15). *With* these refinements, however, this is not necessarily true. Because capabilities decline with distance, for example, it is possible for the first terms in (15) to be negative for both $i$ and $j$. And, since $i$ may count some $k$'s that $j$ does not count (or vice versa) in computing the second term of (15), it is possible that the inequality is not satisfied for either $i$ or $j$.

Thus, the following problem arises in evaluating Bueno de Mesquita's data. Let us consider, for example, a possible war between Guatemala and Bolivia. Suppose that, without the refinements just mentioned, Bolivia is found to be stronger than Guatemala. The null hypothesis would be that each is equally likely to attack the other. If, for reasons quite independent of their relative capabilities, neither is likely to attack the other, this does not matter—the fact that a war does not occur does not count for or against the hypothesis. Suppose, on the other hand, that Bueno de Mesquita's calculations show that $E(U_i) < 0$ for both countries. Then the fact that no war occurs between them would count as evidence for the author's hypothesis, though there are highly plausible alternative explanations.

## The Data

Bueno de Mesquita employs three sets of data to test his theory: one involving interstate threats, one involving interstate interventions, and one involving interstate wars. It is to the last that his theory is most directly applicable, and it is the last that provides him with the most impressive confirmation of it, so it is the last that I propose to examine.

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(These data are presented in Table C, pp. 208-9.) They include 76 wars between 1823 and 1914. Of these, there are only three wars in which $E(U_i) = E(U_j) = 0$. In 48 of the remaining cases, $E(U_i) = -E(U_j)$. And in all cases in which $E(U_i) \neq -E(U_j)$, if one is greater than zero, then the other is less than zero.[3] Thus, we would expect 37 (or 50 percent) of the remaining 73 cases to satisfy Bueno de Mesquita's hypothesis even if there were no relation between the condition he states and the decision to initiate a war. In fact, 62 (or 85 percent) of the aggressors satisfy this condition. The probability of that many observations, or more, occurring in accordance with Bueno de Mesquita's prediction if the null hypothesis were correct is extremely small, so it can be rejected with confidence. But this does not confirm his hypothesis—which was that the satisfaction of (15) is a necessary condition for the initiation of war. We can only infer that a state is less likely to be an aggressor if (15) is not satisfied than if it is.

In the passage quoted earlier, Bueno de Mesquita tries to get around this difficulty by setting arbitrarily high requirements for levels of significance and measures of association in order to confirm that he has identified necessary conditions for war. But this will not work. It is not possible to distinguish between the effects of measurement error and the possible fact that satisfying (15) is not really a necessary condition at all. Moreover, in view of the interdependence between $E(U_i)$ and $E(U_j)$ in his data, the measure of association that he emphasizes (Yule's $Q$) provides no more information than a simple statement of the percentage of correct predictions. But in each case it is a larger number. (In addition, in 10 of his successful predictions, $E(U_i)$ rounds off to .000; in 6 others, $E(U_i) < .05$. Thus, very small changes in measurement would lead to large changes in the percentage of successful predictions.)

Bueno de Mesquita reports that the number of possible wars among countries in his data set in which $E(U_i) \geqq 0$ is significantly below 50 percent of the total (p. 129); he thus argues that the percentage of successful predictions reported is even less likely than I have suggested. This means that there must be many possible wars in which $E(U_i) < 0$ for both potential antagonists; as we have seen, the fact that such wars did not occur is subject to many possible explanations and thus does not provide very strong evidence for Bueno de Mesquita's hypothesis. (The fact that there are no such wars in his data indicates that the wars that actually occurred are not an unbiased sample of all possible wars.)

In any case, this fact would merely add to the already extremely high

[3] I assume that when the author reports $E(U_i) = .000$ and $E(U_i) = -.000$, he means that they are very small numbers (one positive and one negative) that round off to zero. This is true of 10 cases.

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probability that the null hypothesis is incorrect, and thus, that it is wrong to say that there is no relation between the initiation of war and $E(U_i)$. The hypothesis confirmed by Bueno de Mesquita's data, therefore, is the hypothesis that a state is less likely to initiate a war if its capabilities (together with those of its expected allies) are less than those of the expected opposing alliance. That is neither the hypothesis that he stated nor the one he actually tested.

Moreover, it is a version of what Bueno de Mesquita calls a "power politics" hypothesis, which he claims to have discredited (pp. 140-45). But all he has discredited is the assumption that only the capabilities of the aggressor and defender are important in determining the balance of power. His hypothesis, as we have seen, includes the contributions of potential allies in the balance, and his evidence shows that doing so increases the predictive power of the hypothesis. (To be precise, it leads to six additional correct predictions.)

What does expected-utility theory tell us about the hypothesis that Bueno de Mesquita actually tested? It tells us that this hypothesis is wrong: for a state to go to war, it is *not* a necessary condition that the aggregate capabilities of its alliance be greater than the capabilities of its adversaries. If the possible gains from war are sufficiently greater than the possible losses, or if the consequences of not initiating a war are even worse than the consequences of initiating one, then a rational leader will initiate a war even though the probability of losing is greater than the probability of winning. And, of course, a leader may believe that the probability of winning is greater than the probability of losing even though the capabilities of his own side are less than the capabilities of the opposing side.

In practice, these conditions are probably less often satisfied than not; thus, we would expect fewer cases in which aggressors are less powerful than their opponents, than the reverse. That is what expected-utility theory would really lead us to expect, and Bueno de Mesquita's data are consistent with this expectation.

## Conclusions

We have seen that the proposition that foreign policy decisions maximize a leader's expected utility cannot be derived from Bueno de Mesquita's assumption that strong political leaders have a veto over them; that his hypothesis that no state will go to war if $E(U_i)$ (as he defines it) is less than zero cannot be derived from the assumption that foreign policy decisions maximize a political leader's expected utility; that the

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hypothesis he actually tests is more plausibly interpreted as positing that a state will not go to war if its alliance is weaker than the expected opposing alliance; and that the most his evidence shows is that a state is *less likely* to go to war if its alliance is weaker than the opposing alliance, than if it is stronger. Thus, there is a genuine empirical finding reported in *The War Trap*, and it is not inconsistent with the proposition that foreign policy decisions maximize political leaders' expected utility. But the finding cannot be said to confirm that proposition. And, since the hypothesis the author actually tests can be given a straightforward alternative interpretation, the empirical evidence he reports does not imply that his theory must be sounder than is indicated by my analysis of it.

Contrary to appearances, therefore, *The War Trap* provides no evidence, one way or the other, on the question of whether theories of individual rational choice can explain foreign policy decisions. Moreover, when one considers how Bueno de Mesquita's analysis would have to be modified to avoid the criticisms made above, it is apparent that it would not be easy to devise as straightforward a test of this hypothesis as the book professes to provide.

For example, it is simply not credible that an index of two countries' alliance commitments, or any other readily available numerical data, could furnish a means of estimating the utilities that political leaders assign to the possible gains and losses from war or the expected consequences of maintaining current policies. Only Bueno de Mesquita's inconsistent assumptions about those values enable him to avoid confronting this problem. Moreover, any serious attempt to estimate the values leaders place on war would have to face the fact that there are far more than two possible outcomes to any war. And since the consequences of both fighting and not fighting depend upon what the potential enemy decides to do, a game-theoretic analysis would be necessary. Thus, any attempt to state categorically which states' expected utility from war was greater than or equal to their expected utility from continuing current policies seems doomed to failure.

The foregoing does not imply that Bueno de Mesquita is wrong to deplore the divorce between formal theory and systematic empirical research in the study of international politics. It only implies that he has not been completely successful in uniting them; and that more indirect tests of rational choice theories of foreign policy than the one he presents must be devised.

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# ADVANCES IN THE COMPARATIVE STUDY OF ELECTORAL SYSTEMS

By AREND LIJPHART

Michel L. Balinski and H. Peyton Young, *Fair Representation: Meeting the Ideal of One Man, One Vote*. New Haven: Yale University Press, 1982, 191 pp.

Vernon Bogdanor, *The People and the Party System: The Referendum and Electoral Reform in British Politics*. Cambridge: Cambridge University Press, 1981, 285 pp.

Andrew McLaren Carstairs, *A Short History of Electoral Systems in Western Europe*. London: George Allen & Unwin, 1980, 236 pp.

Paul T. David and James W. Ceaser, *Proportional Representation in Presidential Nominating Politics*. Charlottesville: University Press of Virginia, 1980, 298 pp.

G. Gudgin and P. J. Taylor, *Seats, Votes, and the Spatial Organisation of Elections*. London: Pion, 1979, 240 pp.

Geoffrey Hand, Jacques Georgel, and Christoph Sasse, eds., *European Electoral Systems Handbook*. London: Butterworths, 1979, 252 pp.

William P. Irvine, *Does Canada Need a New Electoral System?* Kingston, Ontario: Institute of Intergovernmental Relations, Queen's University, 1981, 100 pp.

R. J. Johnston, *Political, Electoral, and Spatial Systems: An Essay in Political Geography*. Oxford: Clarendon Press, 1979, 221 pp.

Richard S. Katz, *A Theory of Parties and Electoral Systems*. Baltimore: The Johns Hopkins University Press, 1980, 151 pp.

Thomas T. Mackie and Richard Rose, *The International Almanac of Electoral History*, 2d ed. New York: Facts on File, 1982, 422 pp.

Dieter Nohlen, *Wahlsysteme der Welt—Daten und Analysen: Ein Handbuch* [Electoral Systems of the World—Data and Analyses: A Handbook]. Munich: R. Piper, 1978, 449 pp.

Cornelius O'Leary, *Irish Elections, 1918-1977: Parties, Voters and Proportional Representation*. New York: St. Martin's Press, 1979, 134 pp.

THE twelve books under review in this essay illustrate that the comparative analysis of electoral systems is regaining its health. After several decades of apparent moribundity, the first sign of revival was the publication, in 1967, of Douglas W. Rae's masterful *The Political Consequences of Electoral Laws*,[1] but real vigor was not restored until the late 1970s and early 1980s. The scholarly progress demonstrated in

[1] Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967; rev. ed., 1971).

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the twelve volumes is particularly impressive because of its international and multidisciplinary breadth. Of the eighteen authors and editors, nine represent disciplines other than political science: geography (Johnston, Taylor), law (Hand, Georgel, Sasse), history (Carstairs), economics (Gudgin), and mathematics (Balinski and Young). Institutional affiliations are British (Bogdanor, Carstairs, Gudgin, Johnston, Mackie, O'Leary, Rose, Taylor), French (Balinski, Georgel), German (Nohlen, Sasse), Irish (Hand), and Canadian (Irvine); only four of the authors are at U.S. universities (Ceaser, David, Katz, and Young). Moreover, the *European Electoral Systems Handbook* edited by Hand, Georgel, and Sasse contains chapters on the electoral systems of nine of the member-states of the European Communities, each written by a citizen of the country concerned—adding another five nationalities to the multinational roster of authors.

Renewed scholarly attention to the study of electoral systems has been long overdue. The late Stein Rokkan wrote in 1968:

> Given the crucial importance of the organization of legitimate elections in the development of the mass democracies of the twentieth century, it is indeed astounding to discover how little serious effort has been invested in the comparative study of the wealth of information available.

He added that, of the existing literature, "exceedingly little stands up to scrutiny in the light of current standards of social science methodology."[2] In the last two sections of this essay, I will examine the dimensions along which significant improvements have been made since the time of Rokkan's negative judgment of the field—which virtually coincided with the publication of Rae's book—and the considerable problems that remain.

The long neglect of electoral systems by social scientists is all the more surprising because election rules not only have important effects on other elements of the political system (especially the party system), but also because they offer a practical instrument for political engineers who want to make changes in the political system. Giovanni Sartori has aptly characterized electoral systems as "the most specific manipulative instrument of politics."[3] The most likely explanation of the fact that the empirical and practical relevance of election rules has not been more extensively recognized is the widely held assumption that the electoral system is a constant rather than a variable. In this view, electoral laws

[2] Rokkan, "Elections: Electoral Systems," in David L. Sills, ed., *International Encyclopedia of the Social Sciences* (New York: Macmillan, Free Press, 1968), Vol. V, 6-21, at 17.

[3] Sartori, "Political Development and Political Engineering," in John D. Montgomery and Albert O. Hirschman, eds., *Public Policy* (Cambridge: Harvard University Press, 1968), Vol. XVII, 261-98, at 273.

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may be manipulable in principle, but in most countries they tend to be firmly established and very difficult to change in practice.

Richard Katz reasserts this assumption by stating that

> Minor adjustments are to be expected, but major changes should be unlikely. Electoral laws must be changed by politicians who have been successful under the current system. Successful politicians have developed party organizations appropriate to those systems. Clearly then, if reforms are to occur, politicians must be convinced to rise above their self-interest, to change the rules of a game that they are winning.

He maintains that this is likely to happen only in the most extreme circumstances, when a state "seems on the verge of collapse" (p. 123). However plausible this argument appears to be, it is largely incorrect. In the next three sections, I will discuss the relative frequency of both actual and seriously comtemplated major electoral reforms, the direction of these changes toward or away from proportional representation (P.R.), and the current popularity of two models of electoral reform: the Irish single transferable vote and the German "mixed" plurality-P.R. system.

## Major Electoral Reforms

For about 25 years after World War II, the electoral systems of the industrialized democracies appeared to be highly stable and almost immune to change. France's shift from an impure P.R. method to the two-ballot majority-plurality system in 1958 was a striking exception, but it was part of a comprehensive regime change rather than just an electoral reform. The political history of the West European democracies, however, shows that major changes in electoral systems cannot be considered rare. Most of the countries that have proportional representation now did not always have it; they made the switch from majority or plurality systems in the early decades of the 20th century. Carstairs' *A Short History of Electoral Systems in Western Europe* and the handbook by Hand, Georgel, and Sasse give the details of these changes. They also give good descriptions of the frequent shifts France has experienced as a result of "the addiction of French governments to what is commonly referred to as 'electoral engineering' " even without a concomitant regime change—for instance, in 1919, 1927, and 1951 (Carstairs, 176).

Since the early 1970s, there have again been major changes in electoral systems. The single transferable vote form of P.R. was introduced for Northern Ireland's elections in 1973, replacing the plurality single-member district method. France reintroduced P.R. for the election of the French representatives in the European Parliament in 1979 and for

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regional elections in Corsica in 1982. Also in 1982, the Japanese legislature decided to adopt list P.R. for the election of two-fifths of the members of its upper house in place of the single nontransferable vote. The lower house of the Illinois legislature abolished the cumulative vote system in 1980 after it had been in use for more than a century. The presidential nominating process in the United States has also undergone a drastic transformation.

In addition, we should not overlook the reforms that came very close to being adopted. Bogdanor's review of the efforts, between 1831 and 1979, to introduce proportional representation in Britain shows that on two occasions the plurality method was almost abolished. In 1918 it was retained, "in part by default," because the House of Commons favored the alternative vote (a preferential majority system) while the House of Lords voted for P.R. In 1930, another House of Commons majority not only favored the alternative vote, but was determined to override the Lords' opposition. This time, the alternative vote would have been adopted if it had not been for the fall of the government in the 1931 financial crisis. More recently, the British government recommended the list P.R. system, as used in Finland, for the election of Britain's representatives to the European Parliament in 1979. A majority of the governing Labour Party members, joined by the small but unanimous Liberal Party, voted for it; but the Conservatives were strongly opposed, and the proposal was defeated (Bogdanor, 129, 138-39, 166-67).

In Ireland—the country that pioneered the single transferable vote at the national level—proportional representation was nearly abolished on two occasions. O'Leary reports that in 1959 and 1968, parliamentary majorities voted against P.R., but in a referendum the voters decided to retain it both times—although only narrowly the first time (pp. 46-58, 66-70). In the Irish case, P.R. would certainly have been abolished but for two factors: the principle is anchored in the constitution, and constitutional amendments require popular approval by referendum. In most countries, such obstacles to electoral system changes do not exist. In the United States, for instance, nothing in the Constitution prevents Congress from prescribing P.R. for the election of the House of Representatives as long as the electoral districts do not cross state boundaries. The Supreme Court is unlikely to forbid such a change; in fact, the Court would probably applaud proportional representation as being more in line with the "one person, one vote, one value" principle than plurality is.

In recent years, the electoral system has become a highly controversial issue in three of the four Western democracies that use the plurality

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single-member district method. Bogdanor notes that in Britain it is "no longer unthinkable" that plurality might be abolished (p. 3), and the same is true in Canada and New Zealand; the United States is the only exception. One strong stimulus has been the electoral anomaly that the second-largest party in numbers of votes has won a plurality of parliamentary seats, as in Britain in 1974, or even an outright majority, as in New Zealand in 1978 and 1981 (Mackie and Rose, 286-89, 378-81). In Canada the major concern has been the severe regional imbalance in the support of the large parties that is encouraged by plurality elections. For instance, as Irvine points out in *Does Canada Need a New Electoral System?*, the 1980 election yielded a Liberal majority, but with no Liberal member "west and north of Winnipeg, and with half the caucus coming from Quebec" (p. xiii).

The electoral debate in Britain was also stimulated by the first direct election of the European Parliament in 1979 and the commitment by all of the member-states to adopt a uniform electoral system for future elections of this supranational body. The *European Electoral Systems Handbook* arose out of a "research project at the European University Institute at Florence concerned with examining the prospects" for such a uniform system (Hand and others, v). Since almost all of the members of the European Community use proportional representation of some kind, the British are under considerable pressure to conform.

## The Trend toward Proportional Representation

The long-term trend in electoral reform has clearly favored P.R. Of the 21 states that have had democratic forms of government without interruption since the end of the Second World War (those in Western Europe, plus the United States, Canada, Israel, Australia, New Zealand, and Japan), two-thirds use proportional representation for the election of their lower or only houses of the national legislature. Only six (the United States, Canada, Britain, New Zealand, France, and Australia) have plurality or majority systems; Japan uses the single nontransferable vote, a semi-proportional system. P.R. is today's norm; plurality and majority methods are the exception. Much of the movement toward proportional representation took place in the first two decades of the 20th century, but the tendency has reappeared in recent years, as shown by the election reforms in France, Japan, and Northern Ireland. Recent public debates on the desirability of changing the electoral system have been largely confined to non-P.R. countries. Established P.R. systems are rarely challenged; the nonproportional systems—especially plurality—are the ones under attack.

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The main objection to the plurality method has always been that it tends to produce highly disproportional results. In two-party systems, this disproportion can be rather accurately predicted by the cube law: the seat shares tend to correspond to the cube of the vote shares. Malapportionment and gerrymandering may add substantially to the imbalance of the results. In the interaction of these factors, the spatial-geographical dimension is of great importance. The analysis of this dimension is addressed by the two books written or co-authored by geographers: Johnston's *Political, Electoral, and Spatial Systems*, and Gudgin and Taylor's *Seats, Votes, and the Spatial Organisation of Elections* make valuable contributions. Malapportionment is not as serious a problem as gerrymandering, but it can never be satisfactorily solved: "Even if a perfect apportionment were carried out, by the time an election was held the figures upon which the constituencies were based would be out of date. Hence there will always be malapportionment to some degree" (Gudgin and Taylor, 55).

Gerrymandering for partisan advantage can be limited, but not eliminated, by the establishment of neutral commissions that define the electoral districts. In Britain, Johnston finds, "their neutrality is beyond doubt, and yet their effect in most areas is a gerrymander favouring the majority party" (p. 178). Finally, even if both malapportionment and gerrymandering could be completely controlled, the plurality method would still yield disproportional results. Indeed, the cube law is based on the assumption of equal districts and a random delineation of district boundaries (Gudgin and Taylor, 54-55).

It should be remembered, however, that the gerrymander is less an intrinsic feature of the plurality method than of the single-member districts within which plurality is normally applied. It can occur in all systems that have districts from which relatively few representatives are elected. The most notorious example is Ireland, which combines proportional representation with small districts: since 1969, there have been mainly three-member and four-member districts, plus a few five-member districts (O'Leary, 108). Since small districts present the crucial prerequisite for gerrymandering, the "real answer" to the problem, according to Gudgin and Taylor, "seems to be to use larger constituencies. This can be done by raising the minimum size to five-member constituencies thus making all three-member and four-member seats illegal" (p. 179).

Many other problems as well as advantages of the different electoral systems should be taken into consideration—too many to be covered in this essay. A fair and thorough review of these arguments, with

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emphasis on Canada, is found in Irvine's volume (pp. 11-51). His analysis is of special interest to students of election rules, since Canada is the major deviant case in terms of Maurice Duverger's "true sociological law" that plurality methods promote two-party systems.[4]

The one exception to the worldwide trend toward proportional representation is the United States, which seems to be going in the opposite direction. The single transferable vote variation of P.R. has in the past been used for city council elections in about two dozen American cities, but today it survives only in Cambridge, Massachusetts. Illinois abandoned its cumulative vote, a semi-proportional system, in 1980. There is, however, an exception to this exception: proportionality has become the dominant principle in the presidential nominating process since the early 1970s, especially in the Democratic party. The fascinating story of this change in the electoral system—especially significant because it has entailed such a rapid and drastic transformation of the rules—is well told by David and Ceaser in their *Proportional Representation in Presidential Nominating Politics*. It is surprising that this far-reaching change in the intraparty selection process has not prompted any serious examination of a similar modification of the rules for interparty competition. The main justification for the introduction of intraparty proportionality was the same as the argument most frequently used by P.R. advocates: in the words of the McGovern Commission, the "fair representation of minority views" (David and Ceaser, 6). A further irony is that P.R. is normally thought of as a method for the election of multimember representative bodies, but that in the United States it plays a major role only in the selection of a single nominee for a single office.

## The Irish and German Models

Electoral reformers who are dissatisfied with the plurality system tend to favor two alternatives: the single transferable vote, as used in Ireland, and the additional-member system, of which the German Bundestag election is the only national empirical example. The popularity of both of these alternatives is based on the fact that they appear to preserve some of the desirable features of plurality elections. The single transferable vote is a P.R. method, but the voters cast their ballots for individual candidates instead of party lists. The German system is also basically a P.R. procedure, but it preserves the single-member districts from which about half of the Bundestag members are elected. Bogdanor

[4] Duverger, *Political Parties: Their Organization and Activity in the Modern State*, trans. by Barbara and Robert North (New York: Wiley, 1963), 217.

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recommends the single transferable vote for British elections (pp. 232-58), And Irvine proposes that Canada adopt a system inspired by the German model (pp. 52-58).

The popularity of the single transferable vote is somewhat paradoxical because it is rarely used: the normal form of proportional representation is list P.R. In practice the single transferable vote is used for national parliamentary elections only in Ireland, Australia (for the election of the Senate only), and the small country of Malta. That is what makes the Irish case such an important one, and what gives O'Leary's *Irish Elections, 1918–1977* a significance much broader than that of just a single-country study. In spite of its infrequent application, the single transferable vote is the form of P.R. most likely to be adopted eventually by the countries that currently use plurality, if they should ever switch to P.R. It is exactly in these countries that electoral reform has again become a live issue. The single transferable vote has been characterized as the "Anglo-Saxon" form of P.R. (Gudgin and Taylor, 164). In Britain itself, list P.R. was never seriously proposed or considered until the 1976 report of the Hansard Society's Commission on Electoral Reform, which advocated a variant of the German system (Bogdanor, 211).

The single transferable vote has undeniable merits, but these are sometimes exaggerated. Although O'Leary is not among the true believers—he admits that he participated in the 1968 referendum campaign as an advocate of plurality (p. 69)—he does repeat the assertion that "in none of the List systems is the voter allowed to crossvote between parties, as in STV, which may be said to maximise the voter's freedom of choice" (p. 3). In principle, there is no reason why list P.R. systems cannot include a provision permitting voters to split their votes among two or more parties; two countries with list P.R., Switzerland and Luxembourg, actually do give the voters this option, as is pointed out in several of the books under review (Carstairs, 142-43; Hand and others, 175-77; Mackie and Rose, 248, 350; Nohlen, 264, 280). A definite weakness of the Irish system is the small magnitude of the districts, which invites the gerrymander and distorts the proportional translation of votes into seats. But this disadvantage is not inherent in the single transferable vote: adding the requirement of a minimum district magnitude of five seats would make for greater proportionality and would discourage gerrymandering.

The most serious weakness of the single transferable vote is a concomitant of what its supporters see as its main strength: because the choice is among individual candidates instead of party lists, the direct influence of the voters is greater and that of the parties is smaller than

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in list P.R. systems. Moreover, because candidates of the same party have to run against each other, the single transferable vote can be said to promote intraparty democracy. As Katz forcefully points out, however, a high price has to be paid for these "democratic" benefits:

> The genius of mass party democracy as developed in the twentieth century is that it permits average citizens to organize, and so to make up with numbers for what they lack in individual wealth and influence. The intraparty preference vote tends to undo this ... by making politicians dependent on powerful patrons for the resources necessary for electoral competition *within* the party.

Democracy needs competition between strong and cohesive parties, and an electoral system that encourages "party disintegration ... actually may be pernicious" (p. 120; emphasis in original).

The main features of the West German system are: (1) it is a list P.R. system, in which all parties that win at least 5 percent of the national popular vote receive proportional shares of the seats in the Bundestag; (2) half of the seats are also contested in single-member district plurality elections, but the seats won in this manner are simply subtracted from the total number to which the party is entitled according to P.R.; (3) after these district seats have been subtracted, the remaining seats earned by the parties are filled from party lists. This system is a mixture of P.R. and plurality only in the sense that each voter has his or her own district representative in the Bundestag. From the point of view of the overall translation of votes into seats, it is not a mixed but an almost pure P.R. system.

The German example has inspired various proposals, especially in Britain and Canada, for similar additional-member systems. In all of these, the basic idea is to distribute seats at the national or regional level in such a way that those won by plurality in single-member districts plus the additional seats are proportional to the total number of votes received by the parties. Several of the authors who have examined these proposals have reached the important conclusion that relatively many additional seats, such as the 50 percent in the German model, are required to ensure proportionality. For instance, the proposal of the Hansard Society's Commission for 75 percent single-member district seats, topped up by 25 percent additional seats, is not sufficient to guarantee a truly proportional result (Bogdanor, 225-29; Gudgin and Taylor, 192-97; Irvine, 59-67).

The additional-member system did not originate in Germany: it was used in Denmark in 1918–1920 during the brief transition from plurality to proportional representation (Carstairs, 79; Hand and others, 32; Noh-

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len, 229). Subsequently, the Danes continued to use additional members in combination with P.R. in order to iron out the disproportionalities resulting from the application of P.R. in relatively small districts. In these cases, additional seats are used to maximize proportionality; but they are potentially usable for other purposes as well. Gudgin and Taylor raise the intriguing possibility that additional members may be used to guarantee the results that are normally (but not always) produced by plurality—such as the overrepresentation of the largest party and the capture of a majority of seats by the plurality vote winner. They suggest an

> explicit incorporation of a seats-votes function into the electoral law. This may be achieved by using an additional-member system in which the topping-up procedure produces a desired seats-votes relationship that need not be the proportionality produced in German elections ... In Britain the cube law might be used in allocating additional members (p. 198).

The cube rule, an empirical and probabilistic law, can thus be made into a prescriptive law.

## Post-Rae Developments in Research

Without belittling the earlier valuable contributions of scholars like Maurice Duverger, Ferdinand A. Hermens, Enid Lakeman, W.J.M. Mackenzie, and Stein Rokkan, one may say that Rae's work represents the most important modern milestone in the systematic comparative analysis of electoral systems. Virtually all of the books reviewed in this essay pay tribute to Rae and cite his principle findings. Two of the volumes, Katz's and Nohlen's, are explicit efforts to build upon Rae (Katz, 20; Nohlen, 14).

Nohlen's title *Wahlsysteme der Welt* (Electoral Systems of the World) aptly describes his book. In contrast with the 20 Western democracies, mainly in the North Atlantic area, studied by Rae, Nohlen's coverage is worldwide. For instance, he pays detailed attention to the Maltese and Australian examples of the single transferable vote (pp. 322-23), and he discusses unusual methods that do not fit the conventional classification into plurality-majority, proportional, and semi-proportional systems, such as those of Cyprus, Lebanon, and Uruguay (pp. 153-55, 324-29, 351-56). The only omission is that the many countries covered are not listed in the index. Nohlen's conclusions generally confirm Rae's findings (pp. 357-76).

Katz's *A Theory of Parties and Electoral Systems* compares a sample of 14 Western democracies and subjects the parties of a small subset of

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these—Britain, Ireland, and Italy—to an intensive test. Rae's analysis focused on three dimensions of electoral law: the electoral formula, district magnitude, and ballot structure. Katz tries to refine these in several respects and identifies a crucial fourth dimension: intraparty electoral choice. His most interesting conclusions concern this factor: "Where there is an intraparty choice, a significant proportion of the parliamentary turnover will result from intraparty competition rather than interparty competition"; furthermore, "parliamentary parties will tend to be disunited" (p. 34).

When Rae did his research, there were no reliable general compilations of election data and electoral laws; he had to gather his materials laboriously from a variety of sources. For three of the countries in his sample—Australia, Austria, and Switzerland—he was unable to find complete data on party vote totals. He also made a few serious mistakes in his descriptions of electoral formulas; for instance, he classified both the French 1951 formula for the non-Parisian districts and the Luxembourg formula as largest-remainder P.R., whereas they are actually d'Hondt formulas.[5] In both respects, the tools for electoral systems research have improved dramatically.

Today's researchers enjoy easy access to electoral data on about two dozen industrialized democracies by means of *The International Almanac of Electoral History* by Mackie and Rose. The data are reported in terms of the numbers and percentages of votes and seats for all major parties. The first edition of the *Almanac*, published in 1974, has already become an indispensable tool for comparative electoral research. Annual supplements have been included in the September issues of the *European Journal of Political Research*. The second edition extends the coverage to the end of 1981. It also includes three additional countries—Greece, Portugal, and Spain—but deletes Malta and South Africa which were covered in the first edition. The omission of Malta is unfortunate since it is one of the few countries using the single transferable vote. Mackie and Rose state that Malta was dropped "because of its small size" (p. ix); however, Luxembourg, with about the same population as Malta's, and Iceland, with a smaller population, are both retained in the second edition. A more serious gap in both editions of the *Almanac* is the absence of data on the U.S. House of Representatives; for all of the other countries, the national legislative elections are covered in full. The seat distributions in the House can easily be found elsewhere, but there are no other good sources for the national vote totals won by the major parties.

[5] Rae (fn. 1), 42-43, 185-88.

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Since the United States is the largest Western democracy, this is a curious gap indeed.

Mackie and Rose's *Almanac* is also useful in another respect: it provides succinct descriptions of the main features of the electoral laws in each of the country sections as well as in a summary appendix (pp. 406-12). Similar, but more detailed information on electoral laws can be found in Nohlen's handbook, in Carstairs' *Short History*, and—in an admirably systematic way, with each author following a uniform checklist—in the *European Electoral Systems Handbook*.

## Still an Underdeveloped Field

Although much has been accomplished in the comparative study of electoral systems in recent years, much more remains to be done. One big obstacle to further progress is that most political scientists continue to underestimate the importance of electoral systems. In their preface, Gudgin and Taylor complain that their manuscripts were frequently rejected by political science journals; they speculate that the reason was disciplinary jealousy: "opposition to research on elections being done by other than political scientists" (p. x). The more likely explanation is simple indifference or even hostility by political scientists to the subject of electoral systems.

This indifference appears to be greater in the United States than elsewhere. The fact that relatively few of the books reviewed in this essay are by American authors is probably no coincidence; the United States has remained immune to the widespread dissatisfaction with the plurality rule that has infected Britain, Canada, and New Zealand. One exception, mentioned earlier, concerns the presidential nominating process. Another exception arises from the constitutional requirement that House seats be proportionally allocated to the states according to their respective populations. In the more distant past, several prominent Americans have therefore given their attention to the basic problem of proportional representation: what is the most accurate proportional way of translating votes, or voters, into seats?

The most frequently used methods are the d'Hondt, Sainte-Laguë, and largest-remainder formulas, which may result in slightly differing allocations of seats. The P.R. formula named after Victor d'Hondt was actually invented by Thomas Jefferson, and should properly be called the Jefferson method; Daniel Webster is the true originator of the Sainte-Laguë formula. In *Fair Representation*, Balinski and Young review the history of the different apportionment methods for the House of Rep-

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resentatives. They argue that the current method is unfair; it discriminates in favor of the smaller states. The authors provide convincing proof that the one fair method is the Webster formula (pp. 84-86).

Balinski and Young also examine other P.R. systems (pp. 87-93); they confirm in an elegant way some of the earlier empirical findings concerning the differences between P.R. formulas, such as the tendency of the d'Hondt method to favor the larger parties. Although this fact is well known, one can still encounter the erroneous assertion that the d'Hondt system "does not in any way work to the disadvantage of the small parties" (Hand and others, 172). It is typical of an underdeveloped field that knowledge is insufficiently cumulative.

Several other signs of underdevelopment may be noted briefly. One is the absence of a widely accepted basic terminology. For instance, the Irish single transferable vote and the Japanese single nontransferable vote, which are, respectively, a proportional and a semi-proportional system, are sometimes referred to, without much explanation of this unusual classification, as plurality systems (Johnston, 54; Katz, 9, 31, 40, 63; O'Leary, 49; Mackie and Rose, 406). Another example is that Rae's useful term "district magnitude," which denotes the number of representatives elected in a district, and which unambiguously distinguishes this meaning from various other aspects of district size, is ignored by most of the authors under review. It is also symptomatic that the name of Sainte-Laguë is usually misspelled (Balinski and Young, 60, 92; Hand and others, 34, 43; Irvine, 22, 85; Mackie and Rose, 411; Nohlen, 78, 229). Furthermore, comparisons of vote shares and seat shares won by parties continue to use only the "first" votes cast, even in two-ballot and preferential electoral systems, although it would make much better theoretical sense to use the votes on the decisive ballot or in the decisive round of counting. Among our authors, only Carstairs attempts the latter kind of analysis (pp. 40-41).

Despite these minor shortcomings, the twelve books discussed in this essay constitute a promising foundation for further research. The comparative study of electoral systems used to be a completely under-researched field. It is still not well developed, but it has clearly become less underdeveloped.

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# FAILURE, DEFEAT, DEBACLE: U.S. Policy in the Middle East

By LEONARD BINDER*

Joseph Churba, *The Politics of Defeat: America's Decline in the Middle East*. New York: Cyrco Press, 1977, 224 pp.
Wilbur Crane Eveland, *Ropes of Sand: America's Failure in the Middle East*. New York: W. W. Norton, 1980, 382 pp.
Michael Ledeen and William Lewis, *Debacle: The American Failure in Iran*. New York: Knopf, 1981, 256 pp.
Barry Rubin, *Paved with Good Intentions: The American Experience and Iran*. New York: Oxford University Press, 1980, 426 pp.

THE titles of the books occasioning this essay state the matter more succinctly than most: one of the most widely prevalent assumptions in the analysis of U.S. policy in the Middle East is that this country has somehow failed, been defeated, or suffered a severe loss. A second theme, sounded for the most part in recent works on Iran, is that the United States has been ill-informed, misguided, lacking in understanding, or unwilling to use properly the expert knowledge that is available. Generally speaking, the "disasters" that have justified such harsh judgments are the Iranian revolution and the increase in the price of petroleum since 1973. The other disasters that haunt these books are in the future. American writers view with alarm the unfolding events that they attribute to the ineptitude of those charged with the conduct of our foreign policy, and they predict terrible consequences if Washington does not change its ways. Usually the wider critique may be narrowed down, for all practical purposes, to a single issue that motivates the particular author: increased military grants for Israel, recognition of the P.L.O., support for Saudi Arabia, strengthening the Department of State, bringing back Henry Kissinger, or getting rid of the present administration at the next election. Obviously such approaches assume that the government is able to do something about the Middle East, but they tend to slight the situational constraints in the region and to overemphasize the degree to which diplomatic and foreign policy skills can really alter the way things are. To a surprisingly great extent, these authors argue that Washington's conception of Middle East affairs has been shaped

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by ideologically motivated rhetoric, leading U.S. policy makers to false conceptions. The misdirection of American policy, away from "reality" and toward illusion, moves its critics to contempt, to anger, to fear, to derision, and finally to astonishment that repeated experience has not disabused the offending officials of their false consciousness.

It is obviously true that the United States has not had things its own way in the Middle East, but it is unlikely that any amount of skill and knowledge and experience could bring about a situation that could be regarded as ideal. There is, moreover, no consensus on what such a situation would look like, even if there is some measure of agreement on the general goals of American foreign policy in the region. Not only do various groups of interested Americans disagree both in their definitions of the situation and in their preferences, but it is also impossible to please all friends of the U.S. and win over all non-friends in the Middle East all at once. Some authors seem to consider it a major defeat of U.S. policy that warm relations have not been established with every Middle Eastern state. In fact, it is remarkable that this country has won so many friends in the area, while the Soviet Union has lost considerable influence.

The major cause of America's improved position is Egypt's switch from a pro-Soviet policy to a pro-American one. The new Egyptian orientation has outweighed even the importance of the Iranian revolution, and its benefits have continued beyond Egypt's partial excommunication by those Arab states that oppose the Camp David agreements. But the shift in Egypt's policy is not the result of U.S. policy makers' having learned something new and important which they then acted upon. Egypt was able to decide upon a reorientation of policy precisely because the American position remained more or less consistent throughout the War of Attrition and the 1973 War. Were it not for the fact that Egyptian leaders knew where the U.S. stood, they could not have calculated the potential benefit resulting from a policy change. Of course, the administration then had to fulfill those expectations in order to consolidate the change; it had shown itself to be increasingly ready to reward Egyptian cooperation.

The point is not that the U.S. should stand pat at all times, but that the originating cause of important changes was not due to a profound change in Washington's perception of Egyptian realities. Nor is it due to the administration's acceptance of the dictum that it is irrational to attempt to maintain good relations with both Israel and the Arab states, or to its resolution of the dilemma over the respective values of Israeli military power versus Arab oil, territory, and manpower.

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Quite apart from the investment of subjective and ideological preferences with epistemological significance, the kinds of knowledge that U.S. policy makers are expected to have available, and the sorts of predictions they are supposed to make, are quite unrealistic. Obviously, there are hidden agendas behind much of the discussion of the poverty of available information about the Middle East, the weakness of political intelligence, and the frequency with which Washington is surprised by new developments. The implication of all this is that if the foreign policy establishment had had the right knowledge at the start, it could have predicted what was likely to happen; it might then have taken timely action to prevent the undesirable and bring about a more favorable result.

Insofar as this kind of argument emphasizes more efficient and extensive intelligence gathering, by both the overt and covert services, it seems reasonable to assume that the more accurate information one has, the better decisions one ought to make. Still, it does not require much sophistication to realize that a large amount of information requires an efficient processing system, lest decision making bog down in detail. The processing of this kind of information, even when electronically assisted, is anything but a mindless activity, since it depends on some knowledge or theory of what is most relevant in particular political contexts. There is, in fact a systematic ambiguity in many of these attacks on purported American intelligence failures in the Middle East because of the conflation of the kind of knowledge derived from intelligence work and the kind of knowledge obtained from a sound education in the culture, history, language, and institutions of the peoples of the Middle East—not to mention the study of international politics itself. It is one thing to point out the particular facts that were not known—and that others tried to withhold—and it is quite another to condemn either the universities or the media establishment, or both, for failing in their tasks of educating and informing.

There is an even more complicated ambiguity in those criticisms that suggest that the morality self-interest dilemma can be resolved by means of an increased knowledge of the consequences (at least) of certain policies. Since most polemicists are inclined to argue that both international morality and America's self-interest will be served by the policy they advocate, and since they are inclined to maintain that it is possible to offer intellectual proof in support of their position, the scope of the task they impose upon the scholarly study of Middle East politics is formidable—even dismaying—to those of us struggling simply to deal with the question of objectivity.

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A case in point is Joseph Churba's critique of American policy in the Middle East. Churba makes so strong a case for Israel's military and strategic value to the U.S. that the result appears exaggerated, simplified, and one-sided. Whatever merit his argument has is largely dissipated by its unidimensionality, its polemical tone, and its lack of any hesitation in drawing conclusions. According to Churba, the United States is engaged in a policy leading ultimately to defeat because it has not recognized that appeasing the Arabs will eventually result in the neutralization or elimination of Israeli power—America's most efficient military instrument in the region.

Churba's attack on the Middle East policy of the Carter administration is cast in purely military terms, which are defined by the hostility between the U.S. and the U.S.S.R. Insofar as the Middle East has any sort of regional international political structure of its own, that structure militates against the effective use of U.S. power, renders Israel America's only means of using its power in the region, and gives the Soviets a strategic advantage based on proximity and the instability of Middle East regimes. The logical conclusion of such an analysis is that Israel should be encouraged to play a disruptive or intrusive role, to hinder any Arab state from establishing a regional hegemony, to prevent any Arab state from increasing its territory, and to provide facilities for the United States to use its military power both within the region and beyond it. Churba does not question whether the Israeli government or the Israeli people would welcome such a role, or whether they would be inclined to see their security interests defined in terms of a potential conflict between American and Soviet forces in the Indian Ocean.

In effect Churba's analysis is an elaboration of his view that the "vitality of Israel is crucial to the United States and that the United States must therefore categorically commit itself to the defense and preservation of that nation" (p. 17). He believes that the Soviet Union will never relent in its effort to gain control of the Middle East. He also thinks that the dynamics of inter-Arab politics and of Arab political culture will continue to produce instability in the region regardless of any compromise on the Palestine question. Hence, he argues that in spite of détente and the sort of concessions wrung from the Israelis after the 1973 War, the Arab states will seek, with Soviet support, to destroy Israel and thus render the region open to Soviet access, leaving the United States greatly disadvantaged in any contest for control of the Middle East and its huge petroleum reserves.

Churba maintains that Israel "dramatically arrested the expansion of Soviet power" in the War of Attrition (1969–1970) and in the 1973 War

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with Egypt (p. 31). He denies that the Soviet Union has exercised any moderating influence on either Egypt or Syria except to avoid direct military involvement, and he insists that those Americans who believe that the Soviet Union has a "constructive" role to play in the Middle East peace process are gravely misguided.

But if Israel has succeeded in foiling Soviet attempts to increase the U.S.S.R.'s influence in the Middle East, this achievement, according to Churba, has been neither adequately appreciated nor fully rewarded by the United States. Paradoxically, Soviet failures have led both Egypt and Syria to seek an accommodation with the U.S. at Israel's expense. Washington has been duped by the apparent cooperativeness of these two countries and has pressed Israel for political and territorial concessions that will endanger its security and render it incapable of protecting U.S. *interests in the Middle East.*

On the Palestine question, Churba is quite adamant that the establishment of a second Palestinian state between Jordan and Israel will lead to a war in which the Soviets will assist the Palestinians in driving the Israelis into the sea. The destruction of Israel will be followed by the establishment of an Egyptian hegemony over the Fertile Crescent and the consequent loss of independence for the new Palestinian state. Churba concludes his discussion of the Palestine problem with a paradigmatic formulation characteristic of the rhetorical texture of the book: "It is indeed important to reflect that only a strong Israel astride the kingsway of the Middle East can safeguard those necessary conditions on the ground, permitting the Arabs of Palestine on both banks of the Jordan River to maintain an independent national existence" (p. 185).

Admittedly, more than six years after publication, and almost three years into the Reagan administration, this book seems a lot more unbalanced than might have appeared originally. Moreover, the author may have been influenced by the fact that he lost his job as the senior Middle East intelligence analyst for the Air Force because of policy differences. But even now that the Reagan administration has turned away from détente and has deferred the search for a comprehensive solution to the Arab-Israeli dispute, the argument seems overstated and exaggerated.

Since this volume was published, the Egyptian-Israeli peace treaty has been signed; despite many doubts, it has thus far withstood the strains of the invasion of Lebanon and the destruction of the military power of the P.L.O. Egypt under Mubarak does not seem as eager to extend its power to the Fertile Crescent as it is to rebuild good relations with other Arab states.

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The Reagan administration did not oppose the Israeli invasion of Lebanon even though it differed sharply with Ariel Sharon's views on how the fruits of the Israeli military success were to be allocated. Washington has accepted the thesis that Israel is a valuable military asset, and not only in the Middle East; it only briefly and reluctantly pressured Israel by announcing a delay in aircraft deliveries until the Lebanon-Israeli agreement was signed. Above all, the Reagan peace plan of September 1, 1982, rejects the concept of an independent Palestinian state on the West Bank and in the Gaza Strip even while opposing the Israeli annexation of the region. The Reagan formula does not sound so different from the convoluted desiderata expressed by Churba.

In view of the enhancement of the American position in the Middle East since the 1973 War, it is plausible to argue that U.S. military assistance to Israel has resulted in the accrual of significant political and strategic credits, which the United States was then able to reallocate to some of the Arab states in return for their cooperation. Israel and supporters of Israel have bitterly resented the redistributive affect of U.S. policy and the tendency of the U.S. policy makers to exploit Israeli military victories while dissociating themselves from Israeli political goals. Israeli policy makers do not seem to be so worried about the increasing military dependence of Israel on the U.S.; nor is there serious concern about the degree to which Israelis are engaged in performing dubious military and strategic tasks on behalf of the United States in areas outside of the Middle East. Some, like Churba, see a growing American dependence upon Israel's military, but they wonder whether the U.S. is inclined to believe that Israel is an expendable ally that can be discarded after Egypt, Syria, Saudi Arabia, Lebanon, and the Palestinians have been won over.

Despite the great reduction in tension in American-Israeli relations after the signing of the troop withdrawal agreement, many areas of friction remain that are likely to become intensified whether or not Syria continues to defy U.S. preferences. At the heart of these disagreements are Israeli doubts regarding the scope, the level, and the continuity of American support in the future. There are also disagreements about the conditions under which Israeli military strength serves U.S. purposes. There are differences regarding America's willingness to risk relying upon the stability of those Arab regimes that seem ready to cooperate with the U.S. And there are differences regarding the wisdom of linking the Lebanese situation to the resolution of the West Bank problem.

Despite these differences and the increasing political tensions to which they will give rise, there is a great affinity between the present Israeli

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government and the Reagan government: they have in common a conservative political spirit, an emphasis on military and security affairs, and a narrowly legalistic interpretation of moral obligation. Nor is there any doubt that each has served as a kind of model for the other, as the respective leaders have pointed to the manifest superiority of political realism over moralistic compromise in their pursuit of the national interest. Nevertheless, the kind of support that the U.S. now grants Israel is based upon the paradoxical conviction that American interests in the region go far beyond Israel. So the big question is whether the benefits the United States now derives from Israel will necessarily be lost if Israel makes the territorial and political concessions that the Arabs will presumably accept as a basis for peace.

Even if there are other reasons besides the Palestinian question that explain the instability and suspicion of the Arab regimes, Israel's continued existence as a garrison state, its continued administration of the Palestinian population which remains on the West Bank and in Gaza, and its continued service to American strategic interests in other parts of the world may be too great a price for Israel to pay to avoid the risks entailed in making peace. Considering this problem from the Israeli point of view, the Reagan peace plan has measurably reduced those risks by rejecting the claims of the Palestinians to separate, independent statehood. This means that the substantive issue between the United States and Israel is what sort of control Israel will continue to enjoy on the West Bank and in Gaza, and what status Israeli settlers will have in these areas. Under the Reagan plan, it is certain that Israel will retain some presence in these areas, even though annexation is precluded. It seems that the friction between the U.S. and Israel over the Israeli presence in Lebanon belied the fact that only the narrowest of issues separate these two governments.

Eveland's book—though the subtitle evokes the same theme of failure and futility—is not at all a systematic analysis of American foreign policy in the Middle East. The volume is moderately interesting for the insight it gives into the way in which one very ordinary American became involved in Middle Eastern affairs, came to be known as something of an expert in Arab politics, was drawn into some of the misadventures of the C.I.A. in the 1950s, and then faded from the scene.

Normally, when one reads a book as self-indulgent as this, one develops a certain resentment of the author, but that was not the case this time. I must admit some sympathy for the poor fellow who certainly does not look back on his experience during the 1950s with much pride.

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The work has some further interest for us, however, in its description of the genesis of one Middle East specialist. Mr. Eveland, who takes some pride in his kinship with a maternal cousin who was co-author of the post-World War I King-Crane Commission Report, strives to assert some special attachment to the Middle East in addition to his obvious nostalgia. It was this flimsy identity resolution that led him to a year at the Army Language School and the exalted status of an Arabist. Attachment to the embassies in Baghdad and Damascus completed the illusion. Mr. Eveland makes no exaggerated claims for his own expertise, but one gets the impression that he did not disabuse others regarding either the extent of his knowledge or the utility of that area expertise in managing events and personalities. Eveland is a garden variety of the old area hand who never became an "expert" because he was insufficiently educated, but was nevertheless socialized within a bureaucratic structure and nurtured on inside information which had a half-life of about two weeks. One cannot help thinking of Eveland as the victim of a system similar to the one whereby professional football exploits young, strong, and not very prudent athletes.

Still, Eveland learned some Arabic, spent many years in the Middle East, made a living and some friends, and became a part of a culture that vitally influences American perceptions of Middle East politics, of American interests, and of the relationship between the American political process and Middle East policy. It is in the last chapter of his book that Mr. Eveland sums up the perspective he has gained from decades as a Middle East "hand," a view widely shared among those who are rotated into and out of institutional roles with responsibility for advising on and implementing the Middle East policy of the United States.

Eveland's summary conclusions regarding U.S. policy in the Middle East are neither exceptional in themselves nor do they in any particular sense draw on his years of "experience" in the Middle East. He finds that the primary goals of the United States are to keep the Soviets out of the region, and to keep the oil flowing to the West at the best possible prices. In order to achieve these ends, a comprehensive peace must be brought about between the Israelis and the Arabs. The United States must maintain the friendship of the OAPEC members and support the establishment of an independent Palestinian state in a framework of a modification of the 1949 armistice agreements. In a more negative mode, the antitrust laws should be applied vigorously against the multinational petroleum corporations. The U.S. government should save Israel from itself by applying economic sanctions, in the spirit of President Eisen-

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hower if necessary. It should not allow the pro-Israel lobby to influence American policy during the "silly season" of the presidential elections. It should desist from all C.I.A.-ordained clandestine operations and restrict that organization to intelligence gathering only. "History," writes Eveland, "has shown that defense alliances cannot solve our problems in the present vortex of world power politics; nor is there any possibility whatever of Israel's playing a useful part in the direct military or strategic sense" (p. 364).

Nowhere in this lengthy lament does Eveland make clear what he considers to be America's losses and problems. The fall of the Shah's government, the insecurity and high price of petroleum supply, and the Soviet invasion of Afghanistan are all troubling. The failure to find a solution to the Palestine dispute is tragic, but it is difficult to accept the hypothesis that this is a purely American failure, or that the U.S. could not only impose a permanent and stabilizing solution, but that this solution could be of Washington's choosing regardless of the preferences of the parties involved.

Eveland suggests that little has changed in thirty years in the Middle East. He cites a C.I.A. report to President Truman that emphasized the centrality of the Palestinian problem and noted that Israeli military strength and "the skill with which Israeli policy has fragmented the Arab position" were leading to destabilizing outcomes. Somewhat contradictorily, however, the report states "there are signs that the Arab states are individually seeking to reestablish close ties with the West to gain what protection they can from a successful Israel. ... However, the local power superiority and presumed ambitions of Israel prevent a final stabilization of the Near East" (p. 359). This is surely a delicious quote, and one can hardly blame Eveland for using it since it does seem to render his reminiscences more timely. The phrase "final stabilization" as the C.I.A. formulation of an international political goal certainly strikes one as oddly naive, however. In contrast, we note how the C.I.A. report of the reasons why some Arab states have turned to the U.S. reflects Churba's concern with the same phenomenon. If there is any consistency here, is it in an American willingness to use the Israeli threat to prevent any of the Arab states from getting too close to the Soviets? Obviously, U.S. policy has not been consistent over the years: sometimes it has sought to keep the Soviets out altogether, and sometimes it has sought to apply détente to the Middle East. Still, the search for finality may be the actual enemy of stability in the Middle East, which is as open to change as any other part of the world.

If the U.S. misleads itself in the belief that some sort of final stabi-

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lization is possible, it should be asking itself whether that goal is best achieved by firmly excluding the Soviet Union from the region or whether it would be better to find some accommodative arrangement. It is difficult to predict how this issue may have to be faced in the Persian Gulf so long as the outcome of the Iran-Iraq war remains uncertain, but the parameters of Soviet inclusion or exclusion in the Palestine-Israel sector are, by this time, familiar. The issue has been summed up in the juxtaposition of the Camp David approach and the alternative of renewing the Geneva peace conference in the pursuit of a comprehensive agreement involving all of the Arab states and Israel and some form of Palestinian representation, as well as joint participation of the United States and the Soviet Union as convenors and as prospective guarantors of the treaty.

Both Churba and Eveland place great emphasis on the importance of excluding or limiting the Soviet presence in the Middle East; therefore, both must be expected to reject the Geneva conference formula if not the concept of a comprehensive solution. Eveland's position is that the exclusion of the Soviets can only be accomplished by compelling Israel to make concessions on the Palestine issue. Churba argues that such a policy will actually bring about the very condition that the U.S. seeks to avoid.

Up to the time of the signature of the Lebanese-Israeli treaty (May 17, 1983), it appeared that the lesson of the invasion of Lebanon proved Eveland profoundly mistaken and Churba prophetically correct. The indirect, or even inadvertent, American employment of Israel's armed force in Lebanon in order to achieve concessions resulted in the extension of American influence into Lebanon and the diminution of Soviet influence. The initial Soviet response was low-key. Soviet support for the P.L.O. was revealed to be minimal, tentative, and cautious. And if that caution was related to Soviet support for Syria, the Brezhnev government seemed to be quite slow and reluctant in coming to the aid and comfort of its most loyal ally in the region. American foreign policy planners may have concluded that Syria had no place to go but to the U.S. if it wanted relief, not to speak of territorial concessions from the Israelis. They may have thought that all that was needed was a quick Israeli agreement to move out of Lebanon against a future commitment for a peace treaty and normalized relations with Lebanon. Lest the Israelis balk at accepting a post-dated check, Washington could make a very large down payment on a solution to the Palestine issue that the Israelis could not refuse.

It now appears that Yuri Andropov has made some U.S. foreign

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policy experts look bad. There are many plausible reasons why the Soviets failed to respond more vigorously to the Israeli invasion of Lebanon, but the notion that the caution, or confusion, or slothfulness, or distraction, of the declining Brezhnev government represented a permanent or final stabilization of the politics of the Middle East does not seem to have been warranted. Everything keeps changing all the time and, consequently, political strategies must be adjusted all the time—even though Washington may sometimes find itself in a situation where it would like to stop the clock.

For some time now, parts of the U.S. foreign policy establishment have been convinced that it is possible to win over the Syrians just as the Egyptians were won over. In particular, it was hoped that the influence of the Saudis would be helpful in influencing Syria. The questions were how much would have to be squeezed out of the Israelis, and how high the Soviet counter-bid would be. The invasion of Lebanon seemed to lower to very reasonable levels the prospective costs of permanently excluding the Soviet Union, so the appropriate American offers were made to the Lebanese, the Syrians, and the Israelis. Because of the illusion of permanence, a great deal of time was squandered, and the situation in Lebanon has come to look quite different. With powerful Soviet military support, Syria has flatly rejected the American plan, and it is attempting to provoke Israel into a deepening war of attrition in Lebanon at a time when Israeli public opinion desperately wants out. It is, perhaps, only a matter of time until Arafat will be compelled to leave Lebanon and to allow Asad to dominate the P.L.O.

So which is it? Is Churba correct in arguing that the United States should not have tried to use Israeli concessions to win over the Syrians? Is Eveland correct in arguing that the employment of the Israeli military force drives the Arabs into the arms of the Russians? Is the whole idea of excluding the Soviets from the Middle East misguided and dangerous, or has the U.S. merely missed the boat? Has anything really changed in the Middle East over the last thirty years, or is Washington still making the same mistakes?

In the same spirit of tracing everything back to the 1950s, Eveland blames the American "failure" in Iran on the influence of Allen Dulles and Kermit Roosevelt who engineered the 1953 coup that ousted Mossadeq and returned the Shah to his throne. "For twenty-five years, Operation AJAX was cited to justify the frequent, and often disastrous substitution of clandestine and paramilitary operations for conventional diplomacy" (p. 356). The C.I.A.'s responsibility in the matter, to the exclusion of the State Department, the National Security Council, and

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even the academic establishment is not questioned by Eveland, who concludes:

> The extent to which the CIA's covert operators had misjudged the situation in Iran became evident in January, 1979 . . . [when] the CIA [was] left without any useful contacts through which the United States might initiate constructive relations with Iran's revolutionary government (pp. 356-57).

Eveland thus defines the proper role of the C.I.A. in the Middle East in terms of some of his assignments in the 1950s: as an "anti-diplomat," procuring information from the political opposition and establishing rapport with clandestine counter-elite organizations so that the U.S. would still be welcome after the revolution. This preference for a therapeutic over a prophylactic role would be justified if we believed that events such as the Iranian revolution were inevitable—but how is it possible to know such a thing?

Ledeen and Lewis find two major reasons for the American failure in Iran: the moralistic foreign policy conceptions of President Carter and many of his associates, and the lack of adequate knowledge of Iran. They believe that the revolution could have been averted by the timely use of repressive military force on the part of the Shah. Much of their effort goes into explaining why such force was not in fact used; they conclude that Carter and Vance did not wish to be associated with such measures, that American foreign policy was committed to preserving human rights in Iran, and that the Shah believed the U.S. would no longer support him if he resorted to force. Ledeen and Lewis contend that "in their hearts" U.S. policy makers expected and wanted the Shah to use force to preserve his throne and that they were misled in this matter by the limitations of their knowledge. What they lacked was knowledge of the personality of the Shah, of the kind of threat posed by Khomeini, of the dependence of the military on the Shah, of the nature of Iran's process of political development, and of the fact that the Shah was ill with cancer. To avoid a similar situation in the future, the authors argue in favor of a reaffirmation of *realpolitik* and the desirability of including those who have made a life-long career of studying a particular country in the policy-making system.

It is notable that the kind of knowledge Ledeen and Lewis believe was lacking is similar to the kind dispensed in academic settings—seminars on international relations and on oriental cultures—rather than spot intelligence. But their emphasis on the importance of life-long

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specialization in the study of particular countries suggests the subordination of academic specialization to the experience of old hands—of the theoretical to the practical. They do not consider the possible negative consequences of the subjective commitments that such old hands might build up over their long years of getting to know all of the relevant personalities.

Ledeen and Lewis may be correct in their major premise that the Iranian revolution was not inevitable, but it does not follow that its occurrence was due to some failure on the part of the United States. It was not possible for American officials to take responsibility for the Shah's decisions or for his failure to make them. Some would argue that military force was used at least in the case of the Jaleh Square massacre on September 6, 1978, and that the failure to continue with that policy was due to opposition within the government and within the Iranian officer corps.

During the final months of 1978, political efforts concentrated on the attempt to produce a government that might be accepted as a measure of the good faith of the Shah in liberalizing the regime. The uncompromising hostility of the middle classes was thought not to be a foregone conclusion, and the extreme alternatives of repression or abdication were not taken as the most urgent. The appropriate role of the Iranian military could also vary, depending on the choice of regime liberalization, repression, or abdication. Ledeen and Lewis make it apparent that U.S. policy makers were undecided and disagreed virtually from July 1978 to February 1979. The American indecision was matched by that of the Shah and his closest advisers, and culminated in the mission of General Huyser. Ledeen and Lewis add much to our knowledge of this incident, and allow us to draw the conclusion that U.S. policy, by default or not, opted for the liberalizing alternative and convinced the Iranian military leadership to remain in the wings until it was more than certain that no political compromise was possible. It is now evident that it was too late to invoke military intervention after Khomeini arrived in Iran. In fact, it was impossible to preserve the pro-Western Iranian military machine and insulate it from the revolution taking place.

It seems to me that critics of American policy might well argue that Washington should have urged the Shah to use his army to destroy the revolutionaries early on, or urged the army to kill the Shah or compel his abdication early on, in order to establish a pro-Western military regime. Ledeen and Lewis are far too certain that the officer corps, except for a few "sinister" types like Fardoust, was completely committed to the Shah, and they also seem to think that the United States should

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have been similarly committed. They express moral outrage at the administration's treatment of the Shah after the hostages were taken, but they do not seem to be disturbed by the moral confusion that led President Carter to admit the Shah to this country despite the confirmed information that the U.S. embassy might be attacked as a consequence.

Ledeen and Lewis attribute the American "failure" in Iran to personalities, to tactical errors, to lack of information, and to indecisiveness. They do not explain the revolution in terms of secular historical forces. They are inclined to see the Iranian revolution in the light of the C.I.A. intervention in 1953—as an arena of political manipulation in which, on this occasion, the U.S. goofed. But even viewed from their perspective, it is not clear that similar failures could be prevented in the future—unless the U.S. were to adopt the authors' underlying philosophical view regarding the role of force in politics and the character of authentically popular movements.

For Ledeen and Lewis, the Iranian revolution is not a genuinely popular movement. But their views are not very convincing because the book contains many exaggerations, inaccuracies, and speculative assertions. Their references to "Mullahcracy" as fascism and the comparison with Hitlerism is misleading. Khomeini's book on Islamic government is not *Mein Kampf*. Misstatements abound, especially in the first chapter, where the authors state that Iran "had a classical prerevolutionary structure," and that the Shah "was a living legend" (p. 4); that most shahs ruled through a "highly decentralized system," and that the mullahs (generally? universally?) held the view "that the man of religion was the only one endowed with sufficient wisdom to govern according to Allah's light" (p. 5); that Reza Khan had a brilliant military career before becoming the first Pahlavi shah (p. 6); that "it is to be doubted" that Reza Shah campaigned "against the Islamic leaders out of any desire to transform the structures of Iranian society" (pp. 6-7); that Reza Shah never directed "a systematic challenge to the mullahs" (p. 7); that some of the middle class thought the Rastakhiz party was "the first step toward a general democratization of the political system" (p. 29); that Americans tended "to mix with 'the locals' " (p. 32); that the Shah "was not a great oppressor" and that "the Shah was no great modernizer" (p. 34); that within the "Shi'ite leadership there was a secret hierarchy" (p. 108); that Hafez al-Assad of Syria "owed Khomeini a great debt" (p. 110); that "Shariati's message . . . closely coincided with that of Khomeini" (p. 113); that "the entire PLO network that functioned as a support system for Khomeini's movement was itself a piece in the Soviet Union's international chess game" (p. 117); and so on.

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Despite the profusion of misleading statements and partial truths in their exposition, Ledeen and Lewis seem to have grasped the essence of the still mysterious Huyser mission:

> Expected at once to organize and prevent a coup d'état, encourage and restrain the Iranian generals, support Bakhtiar and a military organization that had pledged its loyalty to the shah, and master the subtleties of the Iranian whirlwind, Huyser was out of his element. It is unlikely that anyone could have performed this mission effectively ... (p. 180).

Rubin has written an informative and responsible narrative of the events of the Iranian revolution from the perspective of Washington. Although he states that the revolution was not the result of some failure on the part of the U.S., he also accepts the view of many Iran specialists that the United States consistently failed to probe beneath the surface of Iranian politics. As a consequence, American political leaders failed to recognize the popular basis and emotional intensity of the opposition to the Shah. Rubin traces this attitude back to the Mossadeq era when President Eisenhower was put off by the outlandish political style of the Iranian prime minister.

Rubin's exposition is long on facts and short on analysis. Insofar as there is any underlying explanatory theme, it is not the simple-minded idea retailed by Ledeen and Lewis of a traditional regime whose time had come, but some notion of a severe culture gap between the U.S. foreign policy establishment and the Iranian elite.

Although not much could have been done about the situation, Rubin deplores the fact that the United States was not prepared for what eventually happened. Rubin puts the greatest emphasis on Washington's misunderstanding of the Shah and, like Ledeen and Lewis, he argues that the Shah, ill with cancer, was really a vacillating, uncertain, and hesitant decision maker when severely challenged. Americans were inclined to believe the media image of the Shah, and so misjudged him. Rubin flatly rejects the idea that the Shah's hesitancy was due to President Carter's emphasis on civil rights, but he does not deny that the Shah came to suspect that the U.S. might have wished to get rid of him.

Rubin is best when he discusses the responses of the American bureaucracy to the unfolding revolution, and especially to the taking of the hostages. His recognition that the hostage affair was primarily a matter of the struggle for power in Iran rather than a reflex expression of hatred for the United States is commendable. Despite the best of intentions, Rubin argues, the Carter administration could do little to control the situation because it did not quite understand what was

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happening, and because the political dynamics in both the Huyser incident and the hostage affair were largely Iranian and not transnational. By the time Huyser arrived in Tehran, the leading Iranian generals had already selected their preferred options; no single strategy, let alone one comprising a series of ranked priorities, could be imposed upon them. As for the hostage crisis, that was prefigured in the contacts between Bazargan and Brzezinski in Algiers in the autumn of 1979. Iranian fundamentalists and leftists joined together to weaken the "liberals" represented by Bazargan, Yazdi, Ghotbzadeh, and Bani Sadr. Rubin further notes that the extremists even forced the hand of the leaders of the Islamic Republican party.

Rubin's presentation conveys some of the complexity of the Iranian political arena during the revolutionary process; moreover, he avoids the use of inflammatory labels to characterize our Iranian tormentors. He does not argue that American officials were merely helpless observers, but that the drama was essentially an Iranian tragedy. His good-natured, nonpolemical approach is reflected in his easy, almost journalistic prose style, enlivened throughout by piquant quotes from the major actors in the piece. In fact, the curiosity and excitement with which the world press witnessed the unfolding of the Iranian revolution is with us throughout the book.

Nevertheless, Rubin seems to believe that American officials could have given a better account of themselves. His relatively benign critique is based on the idea that their intentions were good, but that they simply did not understand what was happening. Questions may be raised about both parts of this proposition. It is not clear to me what good intentions mean in this context. The Carter administration was certainly well-intentioned with regard to American interests, but I think Rubin wishes to argue that it thought that the regime of the Shah and his development policies were good for Iran as well. The second part of the proposition implies that, had the Carter administration better understood what was happening, it would have accommodated itself to the inevitable.

Rubin's critique of American policy in Iran, gentle as it is, is directed at two somewhat contradictory defects. He questions the developmental assumptions that dominated the thinking of U.S. officials and led them to such a favorable assessment of the Shah, and he laments the fact that Iran specialists were not adequately used. In accounting for the second point, he explains why experts do not say what they think, how policy makers manage to suppress opinions critical of their preferences, and why more people become Arabists than Iranists. Despite all this elucidation, he ends up stating flatly that American specialists on Iran were

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ignored during the hostage crisis, much to the detriment of Jimmy Carter and the anxiety-ridden American people.

It is clear that none of the authors believe that the defects in U.S. policy in the Middle East can be repaired by training a few more specialists or by mandating the appointment of such specialists to the Policy Planning Council or the National Security Council. The defects are more deeply imbedded in the American system of government and in prevailing dominant ideologies, which are often salient elements in presidential electoral strategies. Whether or not President Carter brought about the fall of the Shah by badgering him on civil rights does not diminish the fact that civil rights was an important theme during the Carter administration. During the Reagan administration, another set of assumptions conditions the possibility of understanding what is going on. American political rhetoric defines the possible limits of any expert knowledge and advice that is to be seriously processed by the system. Those who argue for more reliance upon scholarly expertise, upon the professional intelligence establishment, or upon the experienced "area hand" are calling for consideration of the views of specialists whose training, experience, and professional interests provide them with perspectives that are often opposed to the orientations of political figures and cabinet-level officials. That is not to say, however, that we do not need more and better scholarship, intelligence, and first-hand experience, as well as efficient ways of integrating such contributions with the policy-making process in timely fashion.

There is, nevertheless, a strong theme common to these and other books on U.S. policy which suggests that even if better information were available, policy would have remained the same. The explanation for this condition has to do with the fact that American policy goals are, appropriately, the product of the political process, and are not and ought not be derived from an understanding of the authentic cultural, historical, religious, or even economic destiny of any other country. The role of experts is to ascertain the feasibility and cost of attaining those goals in the light of many factors, not excluding the situation in other regions of the world. The realism/morality issue can be approached as an issue of absolutes or as a problem of prudence, timing, and negotiation; and among the values vying for priority there will also be the question of how far the cultural values of others should be respected. If American policy makers decide against paying the domestic and foreign political price of pursuing foreign policy absolutes, then the questions of feasibility, timing, priorities, and the like can be approached.

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But beyond this platitude—with which we can all agree even if we often forget it—it seems to me that no amount of expertise could tell us whether the United States should have tried (harder?) to prevent the Iranian revolution, for example. Most of the Iran specialists were generally supportive of the revolutionary movement. Several were quite foolishly misled about the clergy, but no more so than were many Iranians. And if the Iranians could be misled, what are we to expect from U.S. experts on Iran? After all, to be an expert on Iran is to be an expert on Iranians. If we expect a higher batting average from the experts, it follows that they should be equipped with more than a profound knowledge of Iranian culture and history and society. But even if it were possible to educate them in the best available knowledge of international relations, military strategy, and political development, they might still favor something like the Iranian revolution because they think it is right, because it has a chance of success, and because they believe that, eventually, the revolutionary impulse will return even if blocked now.

Critics of U.S. policy in Iran argue that, if the experts had been consulted, they would have told the administration whether the revolution could have been prevented (Ledeen and Lewis say it could have, Rubin says it could not); but who was to decide that it should be prevented, if possible, at a bearable cost? Actually, it appears that American policy was not determined in any formalized two-stage process in which goal and method were separated. As Ledeen and Lewis point out, Carter, Vance, and Brzezinski set out to prevent the revolution by the only means that seemed acceptable to them, and ultimately found out that the regime of the Shah could not be kept in power at so low a moral and material price. If the U.S. government had not felt such great remorse after February 1979, it might not subsequently have increased its losses to a level commensurate with its feelings of guilt.

In a similar sense, experts on the Middle East—though they may individually have opinions on the matter—cannot tell us whether Israel is primarily a moral obligation for the United States or a ready instrument of strategic policy. Is Israel an unsinkable aircraft carrier or is it a political embarrassment hampering the development of good relations with all of the Muslim countries? Is it self-defeating for Washington to depend upon and utilize Israeli power, or is it stupid to fail to utilize this great asset? Should the U.S. prevent Israeli territorial expansion at any cost, lest America lose its moral standing, or is our country better served by taking a pragmatic stand on the outcome of the struggle between the Israelis and the Palestinians?

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These are questions that the expert cannot answer because they concern matters of fact and matters of conscience. The role of the expert is to formulate the question accurately and completely, rather than to propose his own answer—and not merely on the issues of the Iranian revolution and support for Israel. With regard to Saudi Arabia, one may ask whether the Saudi regime is really strong, stable, and capable of sustaining the regional policy of a great power, assuring the availability of petroleum in an orderly market, providing the necessary facilities for the defense of the Gulf region, backing conservative, pro-U.S., anti-Soviet governments and movements throughout the region, moderating anti-Western governments, and influencing the mass of pious Sunni Muslims. Or is Saudi Arabia a weak, vulnerable, potentially unstable traditional autocracy which, if it does not fall apart because of its own primitive greed and political simplicity, will tarnish America's moral reputation because of the character of Saudi political culture?

Is Egypt capable of playing the role of hegemonic Arab power, of stabilizing the region, intimidating Libya, supporting Sudan, counterbalancing developments in the Horn of Africa, and providing an important strategic link to the Gulf? Is Egypt able to provide the basis for a successful peace policy? Does the Egyptian elite harbor powerful liberal political and cultural beliefs and commitments that will influence the course of political development in that country and in the region as a whole? Or is Egypt too vulnerable to fanatic mass uprising and terrorist acts, and too burdened with poverty and overpopulation, to play a positive role as a political model? The economic problems of Egypt are so great and the possibilities of any sort of liberal economic solution so limited, that some people believe it to be inevitable that Egypt will return to the Soviet pattern and the Soviet orbit.

The United States is currently (December 1983) engaged in a concerted effort to induce the Jordanian government to join the peace process, to take responsibility for the fate of the West Bank Palestinians, and to represent the P.L.O. Aside from the moral responsibility that Washington must bear should any personal disaster befall King Hussein, American policy makers have to decide whether Jordan will be capable of defending its independence against Syria or Iraq, and whether a Jordanian state dominated by Palestinians would pose a danger to U.S. interests in the region.

Syria, even after the Hama massacre and the Israeli invasion of Lebanon, believed that it had a degree of American support for its role in the Fertile Crescent. There was good reason for such a counterintuitive assumption because on several occasions the U.S. had given tacit approval

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to moves that led Syria to play an expanded role, especially in Lebanon. The assumption that Syria is a responsible actor inclined to control and stabilize the Palestinians wherever it can, and their allies in Lebanon whenever it wishes, is questionable, however. Washington was inclined to believe that Syria would respond affirmatively to the benign influence of Saudi Arabia when necessary, and that the present regime in Syria would remain in power for the time being. Syria has become the greatest threat to the American position in the region.

Nowhere is the confluence and possible contradiction of moral conviction, policy orientation, and expert knowledge more apparent than in the current impasse on Lebanon. The central issue, so compelling that virtually all major actors concur, is the need to restore the sovereign independence and authority of the Lebanese state. Beyond the lip service (which itself evidences a prevailing moral consensus), there is much ambivalence; but it is probably correct to conclude that all of the major protagonists would prefer such a restoration rather than to concede some advantage to their adversaries. There is, of course, the alternative of an accommodation at Lebanon's expense, but even the partition of that unfortunate land would not relieve the burdens of occupation and the threat of war for both Syria and Israel.

The United States, then and now, has denied encouraging the Israeli invasion; one newspaper even quoted Moshe Arens' concurrence in that assertion. The denial may be true with regard to American words but it is questionable with regard to deeds—or at least those of Al Haig. In any case, there is agreement that Washington immediately recognized the "new opportunities" provided by the Israeli military successes. This translates into the strategic consequences of the destruction of the military infrastructure of the P.L.O. in Lebanon as well as the complex domestic and international arrangements and understandings that give quasi-legitimacy to the presence of both the P.L.O. military and the Syrian army in Lebanon. The U.S. tried to avoid moral responsibility for the Israeli invasion, but has labored to construct a new moral frame of reference, replacing the old and permitting the legitimate pursuit of new goals. Obviously, the foremost priority is to prevent the restoration of the *status quo ante*; the middle-range goals include strengthening a potentially friendly regime in Lebanon, restricting Syrian influence, weakening the bargaining power of the P.L.O., strengthening the representational position of Jordan's Hussein, and reducing the scope of Israel's security concerns. Such results would allow the United States to reconstruct the state system in the western part of the Fertile Crescent more or less as it was in 1967, but with the added stabilizing element

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of U.S. guarantees. The moral implications of this policy include the decision to sustain a Maronite Christian dominance in Lebanon, to allow for a minimal adjustment of Lebanon's confessional-constitutional system, to provide no guarantees for the rights of Palestinians as residents in Lebanon, and to absolve the Gemayel government of the responsibility to declare itself not only on the future of its relations with Israel, but also on any divisive issue of interest to its citizens, not excluding the massacre in the Sabra and Shatilla refugee camps.

Even though the area specialists may have been kibbitzing throughout the process by which this complex of policy and gestalt reconstruction was unfolding, their real task begins once this context is in place. They must tell Washington whether the policy will work—principally whether the Gemayel regime will be able to hold the country together, to establish its authority, and to maintain itself not only against internal rivals but against Syrian, Israeli, and Palestinian attempts to influence what goes on within Lebanese boundaries. Their answer, if they are wise, will be hedged with many doubts—in fact the more they know the less certain they will be, and the more they will propose measures that reduce the possibility of failure by increasing the involvement and indirect responsibility of the United States.

For those who oppose the policy, who have different contextual presuppositions, or who start from less pragmatic moral premises, it will be easy enough to point to failures, defeats, and debacles, when things don't work out as Washington would like, but more or less as they expected. The Israeli, the Palestinian, the Jordanian, the Syrian, the Egyptian, and the Saudi authorities have no higher expectations of the Gemayel government than the American authorities do, but they are not as likely to go out on the Lebanese limb. As each one hesitates to take a chance on the Gemayel government, the U.S. must correspondingly increase its promises to each. Instead of constructing some regional arrangement of mutual support and accommodation, Washington has been drawn more and more deeply into a web of bilateral guarantees, commitments, promises, understandings, self-denials, and moral responsibilities.

It seems to me that, given the context of the U.S. policy in the Middle East, pointing to the role of the area expert is a political cop-out. It is an attempt to claim the support of science for one's own political preferences, or to impugn the scientific pretensions of one's opponents. Newsmedia discussions of which Iran specialists made the earliest prediction of the revolution grossly misconstrue both the nature of the knowledge and the role of area experts while demeaning scholarship as a kind of

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competitive sport. Scholars might be willing to accept indignities of this kind if someone would offer multi-million dollar bonuses to new Ph.D.'s, but alas, that is not likely. Under the circumstances, it is perhaps understandable that the area experts themselves have tacitly approved of the misleading discourse that seemed to inflate their value and increase their exposure.

In all of the questions that have arisen, there are not only significant scientific problems, but also matters of moral choice and policy commitment. Washington still supports repressive regimes, and no amount of cultural-relativistic cant should be allowed to obscure the question of America's commitment to its own values. The Iranian revolution raised the question of loyalty to allies and the morality of a foreign policy based on personal rather than national cognizance. The Reagan administration has raised the question of the morality of détente and of consigning the victims of Soviet aggression or subversion to their unhappy fate, but the alternative of a regional military confrontation, even if only by proxy, may be judged even worse. The United States has accepted a great deal of moral responsibility not only for Israel's survival, but also for the survival of King Hussein and the Saudi regime. Even in the matter of Middle Eastern oil, significant moral questions are involved in determining whether the U.S. is engaged in a ruthless economic conflict or should seek to stabilize the price of oil through an accommodative arrangement. And the American public is only gradually becoming aware of the extent to which the Islamic resurgence imposes a moral burden to decide the meaning of tolerance and mutual respect in the face of a cultural challenge that aggressively asserts its rejection of Western values.

Most of us will easily agree that we cannot and ought not turn these issues over to the area experts to decide; but there is a tendency to argue that because these are moral issues, they should be subordinated to the resolution of other problems that are linked to the determination of the facts in the case.

But the facts are no more free of policy assumptions than are policy assumptions free of moral commitment. In no practical sense are the "facts" logically prior to the general policy assumptions that lend meaning to them. The Reagan foreign policy team operates on the basis of a number of related, if tacit, suppositions regarding the way in which an alternate set of suppositions misled the Carter administration into failure, defeat, and finally, debacle. Moreover, these new theses have become increasingly specific and may be readily related to the policies pursued in the Middle East since the release of the hostages. According

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to the Reagan team, the "facts" should be interpreted in the light of the following assumptions:

1. Conflict with the Soviet Union is the supervening structural feature of the Middle East region. Ideology or consciousness (Arab nationalism, Islamic fundamentalism) is the more ubiquitously influential if secondary, feature.
2. Regional problems and bipolar problems can be separated, and can be dealt with differently.
3. The Iranian revolution, the invasion of Afghanistan, and the Iran-Iraq war have further diminished the regional integrity of the Middle East, thus making a U.S. military presence in the area more necessary.
4. The states in the area are not likely to organize a regional alliance against the U.S.S.R.; it is therefore desirable to try to work out arrangements with each country on a bilateral basis.
5. *Regional arrangements in the Middle East generally have an isolationist* flavor; even though they may be justified as neutral or nonaligned, they are frequently manipulated against the U.S.
6. Ideological symbols, whether nationalist, Islamic, socialist, or all three at once, have a decidedly international character, and are aimed at establishing blocs of states that will have as one of their goals at least, the diminution of U.S. influence in the region.
7. Hence, prevalent forms of political consciousness in the Middle East are threatening and require some form of response.
8. Since the U.S. now enjoys a fairly strong position in the Middle East (except in Iran, Syria, South Yemen, and Libya), its general inclination ought to be to preserve and strengthen the status quo, in the sense of upholding the sovereignty and territorial integrity of all states.
9. The U.S. should continue to show a willingness to become a guarantor of treaties, boundaries, regimes, and even the safety of populations in pursuit of the desiderata of a status quo policy, going so far as to interpose itself in a manner that replaces or complements fragmentary or tentative bilateral regional relationships.

If it is true that the U.S. has failed in the Middle East, the failure of the experts or the failure to use the information actually available is only in small part responsible. The context of foreign policy making and advising is set in an ideological and political framework that is more comprehensive and culturally pervasive than is indicated by a mere examination of the bureaucratic structure of the State Department. It is not likely that even the best of expert knowledge could save the administration from policy disasters or compel serious reassessments in midstream.

Nevertheless, it would be useful to have such expert knowledge available outside of the policy-making context and unconstrained by contemporary political controversy, the blandishments of consulting con-

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tracts, or the attractions of commercial deals. It is probably time to reconsider the long-established structure of area studies and their links to universities, foundations, the government, and think tanks; and to explore alternatives such as the establishment of autonomous institutes whose viability will not be tied to the burdensome need to provide introductory courses that masquerade as part of general education, or popular entertainments that are misrepresented as "outreach." If the American people value and need area expertise, why can't it be sought in its own name rather than be dressed up as a general public service to which all citizens are entitled, comparable to social insurance? Middle East studies are caught between the devilish demands of short-run policy exigencies and the deep blue sea of general education. The experts have been too ready to respond to the questions that have been formulated by events—especially unforeseen events of short-run but intense interest. As a consequence, the policy makers have been inclined to treat the larger issues, the forest, in the same manner in which they treat the trees. Nowhere is this inappropriateness of method more apparent than in Washington's approach to what has been called the Islamic resurgence.

Perhaps the most important issue that will have to be faced in the Middle East in the long run is whether it is possible to overcome the increasingly wide gap in cultural understanding between "them" and "us." Religious extremism, the unfortunate image of OPEC, the oddities of family structure, the ubiquity of violence, and the unfamiliarity of the expressions of identity in the Middle East, all contribute to this cultural gap; they nourish suspicion and downright contempt, disdain, and dislike. If the American public, including a large segment of its intellectual elite, did not perceive the Middle East with such fear and disdain, it would not constantly evoke the symbolic cultural insulation provided by experts and scholars whose domain is that of the alien and the exotic, nor would that public almost as often reject the anxious discourse of the area specialists because it mirrors rather than interprets what is already alien.

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**BETWEEN PEACE AND WAR**
The Nature of
International Crisis

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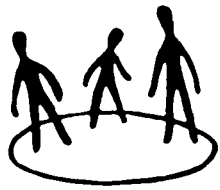

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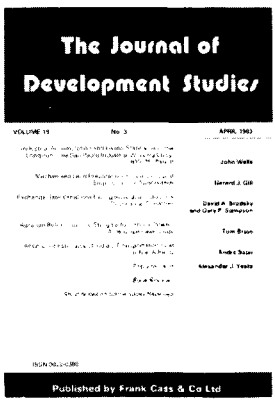


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